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TO DR. POTTER.

DEAR SIR,

I received inclosed in a letter, a few days since from my friend Mr. Chrales Bell, of London, an essay on the anatomy, and physiology of some particular parts of the brain. As this is in all probability, the only copy which has yet reached the United States, it may be gratifying to the faculty to become acquainted as early as possible with the author's views. You are at liberty therefore (if you think proper) to give it a place in your journal. Yours, &c.

WILLIAM GIBSON.

Baltimore, March 13th, 1812.

NOTE.

THE want of any consistent history of the Brain and Nerves, and the dull unmeaning manner in which is in use of demonstrating the brain, may authorize any novelty in the manner of treating the subject.

I have found some of my friends so mistaken in their conception of the object of the demonstrations which I have delivered in my lectures, that I wish to vindicate myself at all hazards. They would have it that I am in search of the seat of the soul; but I wish only to investigate the structure of the brain, as we examine the structure of the eye and ear.

It is not more presumptuous to follow the tracts of nervous matter in the brain, and to attempt to discover the course of sensation, than it is to trace the rays of light through the humours of the eye, and to say, that the retina is the seat of vision. Why are we to close the investigation with the discovery of the external organ?

It would have been easy to have given this essay an imposing splendour, by illustrations and engravings of the parts, but I submit it as a sketch to those who are well able to judge of it in this shape.

The prevailing doctrine of the anatomical schools is, that the whole brain is a common sensorium; that the extremities of the nerves are organized, so that each is fitted to receive a peculiar impression; or that they are distinguished from each other only by delicacy of structure, and by a corresponding delicacy of sensation; that the nerve of the eye, for example, differs from the nerves of touch only in the degree of its sensibility.

It is imagined that impressions, thus differing in kind, are carried along the nerves to the sensorium, and presented to the mind; and that the mind, by the same nerves which receive sensations, sends out the mandate of the will to the moving parts of the body.

It is further imagined, that there is a set of nerves, called vital nerves, which are less strictly connected with the sensorium, or which have upon them knots, cutting off the course of sensation and thereby excluding the vital motions from the government of the will.

This appears sufficiently simple and consistent, until we begin to examine anatomically the structure of the brain, and the course of the nerves,—then all is confusion: the divisions and subdivisions of the brain, the circuitous course of the nerves, their intricate connections, their separation and re-union, are puzzling in the last degree, and are indeed considered as things inscrutable. Thus it is, that he who knows the parts the best, is most in a maze, and he who knows least of anatomy, sees least inconsistency in the commonly received opinion.

In opposition to these opinions, I have to offer reasons for believing—That the cerebrum and cerebellum are different in func-

tion as in form; That the parts of the cerebrum have different functions; and that the nerves which we trace in the body are not single nerves possessing various powers, but bundles of different nerves, whose filaments are united for the convenience of distribution, but which are distinct in office, as they are in origin, from the brain.

That the external organs of the senses have the matter of the nerves adapted to receive certain impressions, while the corresponding organs of the brain are put in activity by the external excitement: That the idea or perception is according to the part of the brain to which the nerve is attached, and that each organ has a certain limited number of changes to be wrought upon it by the external impression:

That the nerves of sense, the nerves of motion, and the vital nerves, are distinct through their whole course, though they seem sometimes united in one bundle; and that they depend for their attributes on the organs of the brain to which they are severally attached.

The view which I have to present, will serve to shew why there are divisions, and many distinct parts in the brain: why some nerves are simple in their origin and distribution, and others intricate beyond description. It will explain the apparently accidental connection betwen the twigs of nerves. It will do away the difficulty of conceiving how sensation and volition should be the operation of the same nerve at the same moment. It will shew how a nerve may lose one property, and retain another; and it will give an interest to the labours of the anatomist in tracing the nerves.

Idea of a new Anatomy of the Brain; submitted for the observations of his friends; by Charles Bell, F.R.S. E.

WHEN, in contemplating the structure of the eye, we say, how admirably it is adapted to the laws of light we use language which implies a partial, and consequently an erroneous view. And the philosopher takes not a more enlarged survey of nature when he declares how curiously the laws of light are adapted to the constitution of the eye.

This creation, of which we are a part, has not been formed in parts. The organ of vision, and the matter or influence carried to the organ, and the qualities of bodies with which we

are acquainted through it, are parts of a system great beyond our imperfect comprehension, formed as it should seem at once in wisdom; not pieced together like the work of human ingenuity.

When this whole was created, (of which the remote planetary system, as well as our bodies, and the objects more familiar to our observation, are but parts) the mind was placed in a body not merely suited to its residence, but in circumstances to be moved by the materials around it; and the capacities of the mind, and the powers of the organs, which are as a medium betwixt the mind and the external world, have an original constitution framed in relation to the qualities of things.

It is admitted that neither bodies nor the images of bodies enter the brain. It is indeed imposible to believe that colour can be conveyed along a nerve; or the vibration in which we suppose sound to consist can be retained in the brain: but we can conceive, and have reason to believe, that an impression is made upon the organs of the outward senses when we see, or hear, or taste.

In this inquiry it is most essential to observe, that while each organ of sense is provided with a capacity of receiving certain changes to be played upon it, as it were, yet each is utterly incapable of receiving the impressions destined for another organ of sensation.

It is also very remarkable that an impression made on two different nerves of sense, though with the same instrument, will produce two distinct sensations; and the ideas resulting will only have relation to the organ affected.

As the announcing of these facts forms a natural introduction to the Anatomy of the Brain, which I am about to deliver, I shall state them more fully.

There are four kinds of Papillæ on the tongue, but with two of those only we have to do at present. Of these, the Papillæ of one kind form the seat of the sense of taste; the other Papillæ (more numerous and smaller) resemble the extremities of the nerves in the common skin, and are the organs of touch

in the tongue. When I take a sharp steel point, and touch one of *these* Papillæ, I feel the sharpness. The sense of *touch* informs me of the shape of the instrument. When I touch a Papilla of taste, I have no sensation similar to the former. I do not know that a point touches the tongue, but I am sensible of a metallic taste, and the sensation passes backward on the tongue.

In the operation of couching the cataract, the pain of piercing the retina with a needle is not so great as that which proceeds from a grain of sand under the eyelid. And although the derangement of the stomach sometimes marks the injury of an organ so delicate, yet the pain is occasioned by piercing the outward coat, not by the affection of the expanded nerve of vision.

If the sensation of light were conveyed to us by the retina, the organ of vision, in consequence of that organ being as much more sensible than the surface of the body as the impression of light is more delicate than that pressure which gives us the sense of touch; what would be the feelings of a man subjected to an operation in which a needle were pushed through the nerve? Life could not bear so great a pain.

But there is an occurence during this operation on the eye which will direct us to the truth; when the needle pierces the eye, the patient has the sensation of a spark of fire before the eye.

This fact is corroborated by experiments made on the eye. When the eye-ball is pressed on the side, we perceive various coloured light. Indeed the mere effect of a blow on the head might inform us, that sensation depends on the exercise of the organ affected, not on the impression conveyed to the external organ; for by the vibration caused hy the blow, the ears ring, and the eye flashes light, while there is neither light, nor sound present.

It may be said, that there is here no proof of the sensation being in the brain more than in the external organ of sense. But when the nerve of a stump is touched, the pain is as if in the amputated extremity. If it be still said that this is no

proper example of a peculiar sense existing without its external organ, I offer the follow example: Quando penis glandem exedat ulcus, et nihil nisi granulatio maneat, ad extremam tamen nervi pudicæ partem ubi terminatur sensus supersunt, et exquisitissima sensus gratificatio.

If light, pressure, galvanism, or electricity produce vision, we must conclude that the idea in the mind is the result of an action excited in the eye or in the brain, not of any thing received, though caused by an impression from without. The operations of the mind are confined not by the limited nature of things created, but by the limited number of our organs of sense. By induction we know that things exist which yet are not brought under the operation of the senses. When we have never known the operation of one of the organs of the five senses, we can never know the ideas pertaining to that sense; and what would be the effect on our minds, even constituted as they now are, with a surperadded organ of sense, no man can distinctly imagine.

As we are parts of the creation, so God has bound us to the material world by this law of our nature, that it shall require excitement from without, and an operation produced by the action of things external to rouse our faculties: But that once brought into activity, the organs can be put in exercise by the mind, and be made to minister to the memory and imagination, and all the faculties of the soul.

I shall hereafter shew, that the operations of the mind are seated in the great mass of the cerebrum, while the parts of the brain to which the nerves of sense tend, strictly form the seat of the sensation, being the internal organs of sense. These organs are operated upon in two directions. They receive the impression from without, as from the eye and ear: and as their action influences the operations of the brain producing perception, so are they brought into action and suffer changes similar to that which they experience from external pressure by the operation of the will; or, as I am now treating of the subject anatomically, by the operation of the great mass of the brain upon them.

In all regulated actions of the muscles we must acknowledge that they are influenced through the same nerves, by the same operation of the sensorium. Now the operations of the body are as nice and curious, and as perfectly regulated before reason has sway, as they are at any time after, when the muscular frame might be supposed to be under the guidance of sense and reason. Instinctive motions are the operations of the same organs, the brain and nerves and muscles, which minister to reason and volition in our mature years. When the young of any animal turns to the nipple, directed by the sense of smelling, the same operations are performed, and through the same means, as afterwards when we make an effort to avoid what is noxious, or desire and move towards what is agreeable.

The operations of the brain may be said to be three fold: 1. The frame of the body is endowed with the characters of life, and the vital parts held together as one system through the operation of the brain and nerves; and the secret operations of the vital organs suffer the controul of the brain, though we are unconscious of the thousand delicate operations which are every instant going on in the body. 2. In the second place, the instinctive motions which precede the development of the intellectual faculties are performed through the brain and nerves. 3. In the last place, the operation of the senses in rouzing the faculties of the mind and the exercise of the mind over the moving parts of the body, is through the brain and nerves. The first of these is perfect in nature, and independent of the mind. The second is a prescribed and limited operation of the instrument of thought and agency. The last begins by imperceptible degrees, and has no limit in extent and variety. It is that to which all the rest is subservient, the end being the calling into activity and the sustaining of an intellectual being.

Thus we see that in as far as is necessary to the great system, the operation of the brain, nerves, and muscles are perfect from the beginning; and we are naturally moved to ask, Might not the operations of the mind have been thus perfect

and spontaneous from the beginning as well as slowly excited into action by outward impressions? Then man would have been an insulated being, not only cut off from the inanimate world around him, but from his fellows; he would have been an individual, not a part of a whole. That he may have a motive and a spring to action, and suffer pain and pleasure, and become an intelligent being, answerable for his actions,—sensation is made to result from external impressions, and reason and passion to come from the experience of good and evil; first as they are in reference to his corporeal frame, and finally as they belong to the intellectual privations and enjoyments.

The brain is a mass of soft matter, in part of a white colour, and generally striated; in part of a grey or cineritious colour, having no fibrous appearance. It has grand divisions and subdivisions: and as the forms exist before the solid bone incloses the brain; and as the distinctions of parts are equally observable in animals whose brain is surrounded with fluid, they evidently are not accidental, but are a consequence of internal structure; or in other words they have a correspondence with distinctions in the uses of the parts of the brain.

On examining the grand divisions of the brain we are forced to admit that there are four brains. For the brain is divided longitudinally by a deep fissure; and the line of distinction can even be traced where the sides are united in substance. Whatever we observe on one side has a corresponding part on the other; and an exact resemblance and symmetry is preserved in all the lateral divisions of the brain. And so, if we take the proof of anatomy, we must admit that as the nerves are double, and the organs of sense double, so is the brain double; and every sensation conveyed to the brain is conveyed to the two lateral parts; and the operations performed must be done in both lateral portions at the same moment.

I speak of the lateral divisions of the brain being distinct brains combined in function, in order the more strongly to mark the distinction betwixt the anterior and posterior grand

divisions. Betwixt the lateral parts there is a strict resemblance in form and substance: each principal part is united by transverse tracts of medullary matter; and there is every provision for their acting with perfect sympathy. On the contrary, the *cerebrum*, the anterior grand division, and the *cerebellum* the posterior grand division, have slight and indirect connexion. In form and division of parts, arrangement of white and grey matter, there is no resemblance. There is here nothing of that symmetry and correspondence of parts which is so remarkable betwixt the right and left portions.

I have found evidence that the vascular system of the cerebellum may be affected independently of the vessels of the cerebrum. I have seen the whole surface of the cerebellum studded with spots of extravasated blood as small as pin heads, so as to be quite red, while no mark of disease was upon the surface of the cerebrum. The action of vessels it is needless to say is under the influence of the parts to which they go; and in this we have a proof of a distinct state of activity in the cerebrum and cerebellum.

From these facts, were there no others, we are entitled to conclude, that in the operations excited in the brain there cannot be such sympathy or corresponding movement in the cerebrum and cerebellum as there is betwixt the lateral portions of the cerebrum; that the anterior and posterior grand divisions of the brain perform distinct offices.

In examining this subject further, we find, when we compare the relative magnitude of the cerebrum to the other parts of the brain in man and in brutes, that in the latter the cerebrum is much smaller, having nothing of the relative magnitude and importance which in man it bears to the other parts of the nervous system; signifying that the cerebrum is the seat of those qualities of mind which distinguish man. We may observe also that the posterior grand division, or *cerebellum* remains more permanent in form; while the cerebrum changes in conformity to the organs of sense, or the endowment of the different classes of animals. In the inferior animals, for example, where there are two external organs of the same

sense, there is to be found two distinct corresponding portions of cerebrum, while the cerebellum corresponds with the frame of the body.

In thinking of this subject, it is natural to expect that we should be able to put the matter to proof by experiment. But how is this to be accomplished, since any experiment direct upon the brain itself must be difficult, if not impossible?—I took this view of the subject. The *medulla spinalis* has a central division, and also a distinction into anterior and posterior fasciculi, corresponding with the anterior and posterior portions of the brain. Further we can trace down the crura of the *cerebrum* into the anterior fasciculus of the spinal marrow and the crura of the *cerebellum* into the posterior fasciculus. I thought that here I might have an opportunity of touching the *cerebellum*, as it were, through the posterior portion of the spinal marrow, and the cerebrum by the anterior portion. To this end I made experiments which, though they were not conclusive, encouraged me in the view I had taken.

I found that injury done to the anterior portion of the spinal marrow, convulsed the animal more certainly than injury done to the posterior portion; but I found it difficult to make the experiment without injuring both portions.

Next considering that the spinal nerves have a double root and being of opinion that the properties of the nerves are derived from their connections with the parts of the brain, I thought that I had an opportunity of putting my opinion to the test of experiment, and of proving at the same time that nerves of different endowments were in the same cord, and held together by the same sheath.

On laying bare the root of the spinal nerves, I found that I could cut across the posterior fasciculus of nerves, which took its origin from the posterior portion of the spinal marrow without convulsing the muscles of the back; but that on touching the anterior fasciculus with the point of the knife, the muscles of the back were immediately convulsed.

Such were my reasons for concluding that the cerebrum and the cerebellum were parts distinct in function, and that

every nerve possessing a double function obtained that by having a double root. I now saw the meaning of the double connexion of the nerves with the spinal marrow; and also the cause of that seeming intricacy in the connexions of nerves throughout their course, which were not double at their origins.

The spinal nerves being double, and having their roots in the spinal marrow, of which a portion comes from the cerebrum and a portion from the cerebellum, they convey the attributes of both grand divisions of the brain to every part; and therefore the distribution of such nerves is simple, one nerve supplying its destined part. But the nerves which come directly from the brain, come from parts of the brain which vary in operation; and in order to bestow different qualities on the parts to which the nerves are distributed, two or more nerves must be united in their course or at their final destination. Hence it is that the first nerve must have branches of the fifth united with it: hence the *portio dura* of the seventh pervades every where the bones of the cranium to unite with the extended branches of the fifth: hence the union of the third and fifth in the orbit: hence the ninth and fifth are both sent to the tongue: hence it is, in short, that no part is sufficiently supplied by one single nerve, unless that nerve be a nerve of the spinal marrow, and have a double root, a connexion (however remotely) with both the cerebrum and cerebellum.

Such nerves as are single in their origin from the spinal marrow will be found either to unite in their course with some other nerve, or to be such as are acknowledged to be peculiar in their operation.

The eighth nerve is from the portion of the *medulla oblongata** which belongs to the cerebellum : the ninth nerve comes from the portion which belongs to the cerebrum. The first is a nerve of the class called Vital nerves, controuling secretly the operation of the body; the last is the Motor nerve of the tongue, and is an instrument of volition. Now the con-

* The *medulla oblongata* is only the commencement of the spinal marrow.

nexions formed by the eighth nerve in its course to the viscera are endless; it seems no where sufficient for the entire purpose of a nerve; for every where it is accompanied by others, and the ninth passes to the tongue, which is already profusely supplied by the fifth.

Understanding the origin of the nerves in the brain to be the source of their powers, we look upon the connexions formed betwixt distant nerves, and upon the combination of nerves in their passage, with some interest; but without this the whole is an unmeaning tissue. Seeing the seeming irregularity in one subject, we say it is accident; but finding that the connexions never vary, we say only that it is strange, until we come to understand the necessity of nerves being combined in order to bestow distinct qualities on the parts to which they are sent.

The *cerebellum* when compared with the *cerebrum* is simple in its form. It has no internal tubercles or masses of cineritious matter in it. The medullary matter comes down from the cineritious cortex, and forms the *crus;* and the *crus* runs into union with the same process from the cerebrum; and they together form the *medulla spinalis*, and are continued down into the spinal marrow; and these crura or processes afford double origin to the double nerves of the spine. The nerves proceeding from the Crus Cerebelli go every where (in seeming union with those from the Crus Cerebri); they unite the body together, and controul the actions of the bodily frame; and especially govern the operation of the viscera necessary to the continuance of life.

In all animals having a nervous system, the *cerebellum* is apparent, even though there be no *cerebrum*. The cerebrum is seen in such tribes of animals as have organs of sense, and it is seen to be near the eyes, or principal organ of sense; and sometimes it is quite separate from the *cerebellum*.

The cerebrum I consider as the grand organ by which the mind is united to the body. Into it all the nerves from the external organs of the senses enter; and from it all the nerves which are agents of the will pass out.

If this be not at once obvious, it proceeds only from the circumstance that the nerves take their origin from the different parts of the brain; and while those nerves are considered as simple cords, this circumstance stands opposed to the conclusion which otherways would be drawn. A nerve having several roots, implies that it propagates its sensation, to the brain generally. But when we find that the several roots are distinct in their endowments, and are in respect to office distinct nerves; then the conclusion is unavoidable, that the portions of the brain are distinct organs of different functions.

To arrive at any understanding of the internal parts of the cerebrum, we must keep in view the relation of the nerves, and must class and distinguish the nerves, and follow them into its substance. If all ideas originate in the mind from external impulse, how can we better investigate the structure of the brain than by following the nerves, which are the means of communication betwixt the brain and the outward organs of the senses?

The nerves of sense, the olfactory, the optic, the auditory, and the gustatory nerve, are traced backwards into certain tubercles or convex bodies in the base of the brain. And I may say, that the nerves of sense either form tubercles before entering the brain, or they enter into those convexities in the base of the *cerebrum*. These convexities are the constituent parts of the cerebrum, and are in all animals necessary parts of the organs of sense: for as certainly as we discover an animal to have an external organ of sense, we find also a medullary tubercle; whilst the superiority of animals in intelligence is shewn by the greater magnitude of the hemispheres or upper part of the cerebrum.

The convex bodies which are seated in the lower part of the cerebrum, and into which the nerves of sense enter, have extensive connexion with the hemispheres on their upper part. From the medullary matter of the hemispheres, again, there pass down, converging to the crura, Striæ, which is the medullary matter taking upon it the character of a nerve; for

from the Crura Cerebri, or its prolongation in the anterior fasciculi of the spinal marrow, go off the nerves of motion.

But with these nerves of motion which are passing outward there are nerves going inwards; nerves from the surfaces of the body; nerves of touch; and nerves of peculiar sensibility, having their seat in the body or viscera. It is not improbable that the tracts of cineritious matter which we observe in the course of the medullary matter of the brain, are the seat of such peculiar sensibilities; the organs of certain powers which seem resident in the body.

As we proceed further in the investigation of the function of the brain, the discussion becomes more hypothetical. But surely physiologists have been mistaken in supposing it necessary to prove sensibility in those parts of the brain which they are to suppose the seat of the intellectual operations. We are not to expeet the same phenomena to result from the cutting or tearing of the brain as from the injury to the nerves. The function of the one is to transmit sensation; the other has a higher operation. The nature of the organs of sense is different; the sensibilities of the parts of the body are various. If the needle piercing the retina during the operation of couching gives no remarkable pain, except in touching the common coats of the eye, ought we to imagine that the seat of the higher operations of the mind should, when injured, exhibit the same effects with the irritation of a nerve? So far therfore from thinking the parts of the brain which are insensible, to be parts inferior (as every part has its use), I should even from this be led to imagine that they had a higher office. And if there be certain parts of the brain which are insensible, and other parts which being injured shake the animal with convulsions exhibiting phenomena similar to those of a wounded nerve, it seems to follow that the latter parts which are endowed with sensibility like the nerves are similar to them in function and use; while the parts of the brain which possess no such sensibility are different in function and organization from the nerves, and have a distinct and higher operation to perform.

If in examining the apparent structure of the brain, we find a part consisting of white medullary Striæ and fasciculated like a nerve, we should conclude that as the use of a nerve is to transmit sensation, not to perform any more peculiar function, such tracts of matter are media of communication, connecting the parts of the brain; rather than the brain itself performing the more peculiar functions. On the other hand, if masses are found in the brain unlike the matter of the nerve, and which yet occupy a place guarded as an organ of importance, we may presume that such parts have a use different from that of merely conveying sensation; we may rather look upon such parts as the seat of the higher powers.

Again, if those parts of the brain which are directly connected with the nerves, and which resemble them in structure, give pain when injured, and occasion convulsion to the animal as the nerves do when they are injured; and if on the contrary such parts as are more remote from the nerves, and of different structures produce no such effect when injured, we may conclude, that the office of the latter parts is more allied to the intellectual operations, less to mere sensation.

I have found at different times all the internal parts of the brain diseased without loss of sense; but I have never seen disease general on the surfaces of the hemispheres without derangement or oppression of the mind during the patient's life. In the case of derangement of mind, falling into lethargy and stupidity, I have constantly found the surface of the hemispheres dry and preternaturally firm, the membrane separating from it with unusual facility.

If I be correct in this view of the subject, then the experiments which have been made upon the brain tend to confirm the conclusions which I should be inclined to draw from strict anatomy; viz. that the cineritious and superficial parts of the brain are the seat of the intellectual functions. For it is found that the surface of the brain is totally, insensible, but that the deep and medullary part being wounded, the animal is convulsed and pained.

At first it is difficult to comprehend, how the part to which every sensation is referred, and by means of which we be-

come acquainted with the various sensations, can itself be insensible; but the consideration of the wide difference of function betwixt a part destined to receive impressions, and a part which is the seat of intellect, reconciles us to the phenomenon. It would be rather strange to find, that there were no distinction exhibited in experiments on parts evidently so different in function as the organs of the senses, the nerves, and the brain. Whether there be a difference in the matter of the nervous system, or a distinction in organization, is of little importance to our enquiries, when it is proved that their essential properties are different, though their union and co-operation be necessary to the completion of their function—the development of their faculties by impulse from external matter.

All ideas originate in the brain: the operation producing them is the remote effect of an agitation or impression on the extremities of the nerves of sense; directly they are consequences of a change or operation in the proper organ of the sense which constitutes a part of the brain, and over these organs, once brought into action by external impulse, the mind has influence. It is provided, that the extremities of the nerves of the senses shall be susceptible each of certain qualities in matter; and betwixt the impression of the outward sense, as it may be called, and the exercise of the internal organ, there is established a connexion by which the ideas excited have a permanent correspondence with the qualities of bodies which surround us.

From the cineritious matter, which is chiefly external, and forming the surface of the cerebrum; and from the grand center of medullary matter of the cerebrum, what are called the *crura* descend. These are fasciculated processes of the cerebrum, from which go off the nerves of motion, the nerves governing the muscular frame. Through the nerves of sense, the *sensorium*, receives impression, but the will is expressed through the medium of the nerves of motion. The secret operations of the bodily frame, and the connexions which unite the parts of the body into system, are through the cerebellum and nerves proceeding from it.

SANFORD'S

Additional observations on the Peruvian Bark.

SIR,

MY son, called at your office in August last, in hopes of having a personal interview with you: intending at the same time, to present to you specimens of the different Barks and their preparations referred to in my printed Letter; but in consequence of your absence he had not the opportunity of making those explanations concerning them, which to him seemed necessary and therefore left the letter only. I have therefore thought proper to forward those specimens, together with some remarks which seem necessary to the further illustration of the subject. Should the specimens of bark now presented, and the explanations given of them in my letter, serve to elucidate enquiries concerning this branch of the Materia Medica; my views in laying them before you will be fully answered. And should the remarks now submitted be thought by you to deserve a place in the Baltimore Medical and Philosophical Lycæum, your insertion of them in that useful work will prove to me satisfactory.

The box which covers this, contains the principal Barks which I have subjected to experiment and enquiry; and I have strong reasons for believing that this collection comprehends nearly every variety of Peruvian Bark of any importance, which is now to be found within the United States.

I regret that the samples of No. 1 and of No. 3, are so limited, the former could be procured only from a practitioner, residing in Virginia, and was forwarded in a letter. The latter has been handed out to friends, until the quantity left is very small. These specimens will however serve to shew the colour and configuration of each.

The powdered sample of No. 1, has been on hand and exposed to the air, for nearly ten years, and is not in its pre-

sent condition thought to be very active. All the others are recent and deemed to be in a good state of preservation.

I am unable to furnish any specimens of No. 2, or of No. 7, which are referred to in my printed letter, there being none to be found in market.

The objects held up to view in my letter on the subject of the bark, are naturally arranged under the following heads, to wit:

First. To establish this fact, that several varieties of bark have lately been brought into use, differing materially from those kinds which had heretofore been known and approved; and that many of those possess but feeble medicinal powers, whilst others are totally destitute of any properties nearly resembling the true bark.

Second. To point out the characteristic appearances of each kind both in the crude and pulverized state in such manner as to render them most easily distinguishable by the senses.

Third. To shew that the middle coating or that part lying between the epidermis and semi-ligneous or cortical, where the glands reside, and the fluids are digested and elaborated, is that portion which contains the chief active principles of the bark; and to prove that this glandular or parenchymatous portion is capable by mechanical means, of being separated in a good degree from the remainder.

Fourth. To ascertain their comparative medicinal powers, as well by noting their effects on the diseased subject, as by the quantity of soluble principle which each affords to given menstrua.

Fifth. To ascertain by chemical tests, the analogy which subsists between the dry extract or essential salt of bark and the saturated infusion, from which it is obtained; and shew by deductions from actual practice the medicinal value of this preparation.

Sixth. To inculcate the propriety and importance of administering the bark in the atonic state of disease in proportion to the lost excitement; and shew that this object might

generally be attained by adequate injections of this substance, even when the stomach would not retain it.

It seems now necessary to state only the result of subsequent observations made respecting the medical use of the essential salt of bark; to describe more particularly the manner of preparing this article; and to add some further particulars by which a Cincona may be most readily and accurately distinguished from every other vegetable substance.

The diseases which have occurred during the late summer and autumn, have furnished considerable opportunity for further use of the essential salt of bark, the effects observed in the treatment of intermitting fevers, correspond nearly with the opinion given of this preparation in my printed letter. It ought however to be remarked, that fevers of this description which were checked by this extract, were observed to recur more speedily after it was discontinued and generally in a greater proportion of cases than when the powder was liberally employed.

I readily concur in opinion with Vanquelin, that the effects produced by the essential salt are not proportional to the bark from which it is taken; nevertheless I think that this preparation ought to be deemed of considerable importance. I have several times witnessed its great efficacy in the last stages of cholera infantum, as well as in other infantile complaints, of both acute and chronic forms. As it is soluble in nearly the same quantity of water as sugar, it is remarkably convenient for the use of children. It sits easier on the stomach, and is less liable to prove cathartic, than the powder. Owing to its small bulk and convenient form for exhibition in pills, it can be used in a disguised form, where the powder would be rejected.

I have known it produce very happy effects, given in this way, where the naming of bark occasioned the most distressing loathing. The extract when dissolved in much water, and kept in a warm situation is speedily decomposed and becomes mouldy; therefore aqueous solutions of this substance ought to be used (especially in summer) within two or three days after being made.

In the preparation of the concrete or essential salts of the bark great care and delicacy of management are required through every stage of the process.

The cold infusion ought to remain on the pulverized bark, until the liquor is very transparent, say from twenty to thirty hours; the sesond infusion from thirty to forty, and the third from forty to fifty hours. The clear liquor is then to be decanted, and after being filtered through several folds of fine flannel or bibulous paper, is to be evaporated in earthen plates, in a degree of heat which is not painful to the hand of the operator, until about one half the quantity be reduced. At this point of evaporation the liquor ought to be decanted with a steady hand into other plates to free it from an earthy looking sediment, which is thrown down by the heat. This part of the operation, must be repeated, say once or twice more, or as often as further sediment appear. When the liquor is reduced to the consistence of thick syrup, the plates containing it are to be removed from the bottom of the furnace, and placed above on slatted scaffolds, until the salt is dry and breaks up in brilliant scales.

It is only in a cool and dry atmosphere, that this substance can be removed from the plates in a dry state, by reason of its great eagerness to absorb moisture. Infusions of bark undergo the putrefactive fermentation during the summer months in so short a time, that it frequently will happen that the first infusion only can be improved. For this reason as well as on account of the deliquescent property of the salts, the temperate seasons are to be preferred for manufacturing this article.

I have hitherto confined my operations to the preparation of the dry extract by slow evaporation according to the plan of the Count De Garaye, its inventor; and which that author and others have denominated the essential salt of the bark.

Should opportunity permit, I propose hereafter to attempt the preparation of the salts in the white and chrystalized form, as described by Vanquelin, to be done by the Deschamps in France. I design likewise to prosecute enquiries

already begun respecting the properties and use of the dry extract of bark, which is obtained by means of Alcohol. The result of which trials will in due time be submitted.

Until the experiments of Monsieur Vanquelin fell into my hands, I was persuaded that every bark, which, when treated with water, precipitated galls, and not animal glue, was changed by solutions of iron to a darkish green colour, and imparted little or no colour to cold water, was a Cincona, and that such vegetable substances as did not afford these precise appearances when treated in the same way, contained little or none of the kinic principle. I was led to this conclusion because it was supported by some respectable authorities, and because every genuine bark, of known and approved virtues, of whatever colour, which I had examined, afforded these phenomena in a conspicuous manner; and because other barks resembling the Peruvian in their sensible properties of bitterness and astringency (of which kind are the barks of the Cornus Florida, and Salix Alba) will not precipitate galls, and turn iron green, but will form a copious precipitate with gelatin.

The Angustura and the lately introduced bark under the name of Alcornoque, are the only two kinds which I have found when tested as above, to agree nearly with the Peruvian bark. These being of the growth of the same country with the Peruvian bark, and being equally tonic, and perhaps febrifuge too, it is not unlikely that by further experience they will be found to belong to the family of the Cincona. If the chemical experiments of Vauquelin are correctly stated, and those barks which he found to precipitate Isinglass and not Galls; and also to give out to cold water high colours, are barks containing the febrifuge principle as he intimates; it would seem that the tests here contended for were made doubtful; or that a highly predominating astringency was in these particular barks held in combination with the kinic principle. But as the author has furnished no satisfactory evidence, derived from actual observation of their medicinal effects on the human system; and as many other astringent

barks which possess no febrifuge power, agree with them in chemical results; it may with propriety be doubted whether these supposed Peruvian barks, have a stronger claim to our confidence as febrifuge medicines, than the bark of the Oak, Persimmon, and numerous others. I think it may therefore be safely assumed as before stated, that every bark whose cold infusion is not higher coloured than a faint Orange, which forms a copious whitish precipitate, with the infusion of galls, little or none with solutions of Isinglass, and which is changed by a solution of red or green sulphate of iron, to a darkish green (not black or brown) is the true Cincona.

The best red bark marked No 1, and the first quality of yellow No. 5, produce when treated alike, results so similar to each other in regard to appearance of precipitate and colour of fluids, that they are not in this way easily distinguished.

The long time required for the powder of the red bark to subside from its containing water, probably has occasioned some mistake as to the colour of its cold infusion.

It is highly desirable that some means should be discovered of ascertaining the medicinal powers of different kinds of Peruvian bark, with greater certainty than can be done by the taste and other senses; and more readily than by observing the quantity of extract which they afford, or by the still more tedious means of observing their effects on the diseased body. I had hoped that those agents which serve to identify the principles sought for, would likewise shew the quantity of those principles. But I have not as yet been able to ascertain the quantity, by means of such of them as shew the existence of the principle. For infusions of bark, whether they be made strong or weak, afford with galls nearly the same quantity of precipitate; and weak infusions when united with iron, do not differ visibly in colour, or otherwise, from stronger ones treated in the same way. This branch of our enquiry in particular, requires further investigation; and I hope it will be prosecuted by those who have

more leisure, than commonly falls to the lot of a country practitioner.

With sentiments of great esteem,

I remain,

Your obedient servant,

CLARK SANFORD,

Greenwich, Connecticut, 7th January, 1812.

Doctor Nathaniel Potter.

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To the Editor of the Baltimore Medical and Phitosophical Lycæum.

Washington, 3d March, 1812.

SIR—Having lately received by the public ship Constitution from France, various intelligence, particularly of a medical and scientific kind, I send you, for the information of the learned faculty of Maryland, an article contained in the *Journal de Medecine* for September last. It is presented to you in the form of translation and abstract; and is intended to introduce if possible, something preferable in the case stated, to the fashionable remedy quicksilver. With great esteem and regard yours,

SAMUEL L. MITCHILL.

—

Cure of Syphylis by Alkalies.

Professor Percy of Munich, has published a tract on the composition of the anti-syphylitic tincture of M. Besnard.—This B. is a knight of the civil order of merit, under the crown of Bavaria, chief of the general inspection of hospitals there, and member of the medical committee, &c. &c. &c. He has made known the secret of a thirty years practice in the treatment of lues. Its composition is the following:—

Take of purified salt of tartar, and

Of simple cinnamon water, each, one pound;

Of the purest opium, two ounces;

Of cinnamon water with wine, four ounces;

Let them be separately dissolved, and afterwards mixed

together, and exposed to a water bath, for three weeks, frequently stiring them.

Of choice gum arabic, two ounces;

Of volatile alkaline salt: one ounce;

Dissolve them in simple cinnamon water, six ounces;

Mix them all together, and let them stand in a well stopped vessel, for some days, at rest, and then be filtered and kept for use.

This medicine is employed for the removal of the venereal disease in all its forms and stages. Its dose is twenty-four drops, thrice in twenty-four hours, in a cup of the decoction of mallows roots, cool. The dose is diminished, when in the course of a few days the symptoms shall be mitigated; until at length, twice or even once a day, will be sufficient for its exhibition.

The quantity must be proportioned to the age and strength of young persons and children. The vehicle for such may be, the syrups of maidenhair, or the milk of almonds.

Where there are local affections, such as venereal ulcers, knobs, lumps, excrescences, phymosis, paraphymosis, and the like, the external application of the remedy, must be added to the internal. And for this purpose a decoction of marsh mallows roots with the same quantity of tincture as was described in the beginning, is to be employed in the form of a bath, embrocation, lotion, gargle, or topical application, according to the nature of the part affected.

Where there are mere ulcerations, they must be moistened, by means of a brush, or pencil, and if they do not heal under the action of the before-mentioned mixture, they must be touched daily with lapis infernalis, and dressed with lint.

If the sores treated in this manner, although clean and not spreading, heal and consolidate but slowly, recourse should be had to restorative diet and strengthening medicines.

Buboes are to be moistened twice a day, with the same tincture, and after some days are employed after this manner, they are to be covered with a plaster made of one ounce

of diachylon cum gum. and half a dram of castile soap, until the swelling disappear, or suppuration ensue.

The same plaster is good for the swelling of the testicles arising from repressed gonorrhæa. Yet, in cases of this kind the suspensors must not be neglected.

In the inflammatory stage of gonorrhæa, the affected part should be bathed as before directed; and four pounds of the mallows decoction, and twenty four drops of the tincture be taken daily into the stomach. The inflammation being thus subdued, the tincture is to be taken three times a day as first directed in the mallows decoction; and at the same time, injections made of a dram of pure alum, four ounces of the mucilage of gum arabic, and a pound of distiled water, may be employed, to the urethra in men and the vagina in women, three or four times a day, and if in the progress of the cure the discharge does not cease, a scruple of the extract of lead, may be added to the remedy.

The recovery of the patient will always be expedited, by bathing the whole body in warm water, impregnated with a measure of common lee, or a portion of potash, as well as by a proper regard to food and drink.

As to other symptoms, when they occur, the physician must treat them according to his judgment, and the rules of art.

Such is the practice in this formidable malady, which is reported to have extended over a great part of Germany, the departments of the Rhine, the military hospitals of Bavaria. and the kingdom of the same name.

M. Besnard is reported to be the archiator, or first physician to Maximilian Joseph, King of Bavaria. By publishing this remedy, he is extolled as having bequeathed a rich legacy to suffering humanity. His sovereign has issued a certificate or proclamation in these words. "Maximilian Joseph; by the grace of God, king of Bavaria, &c.; after the great number of successful trials made in different places, and by different physicians and surgeons, with the anti-sy-

phylific tincture of M. de Besnard, in all sorts of venereal diseases, whereon is founded the definitive conclusion mentioned in the report of the administrative direction of Bavaria; We testify by these presents, to its inventor M. de Besnard, chief of the general inspection of all the military hospitals, our high approbation of the important service he has rendered to humanity, and we further assure him of our entire good will, and of our sovereign protection."

"*Munich, 8th May,* 1808." (Signed.)

It will immediately appear to the experienced practitioner, that the important and essential ingredients of this composition are the POTASH and OPIUM. The cinnamon water, the gum arabic and even the ammoniac appear to be of little value in the receipt, and as it is roundly affirmed that M. de B. has long since discarded the use of mercury in the cure of syphylis, let the form of it be as it might and the mask ever so imposing and deceptive, we may conclude that the *fixed alkaline salt* and the *juice of the poppy*, are capable of eradicating this formidable disease.

It may be also noticed, that if this anti-syphylitic preparation should be considered as novel, the source whence he derived it will not be deemed so. It will immediately occur to the well read physician, that *Professor Peyrilhe*, of Paris, attempted as long ago as 1769, to substitute the alkaline for the mercurial practice; and that in 1780, he published a book on the advantages of that method of treatment. The curious and pungent matter which it contained, raised against him a host of critics, detractors, and enemies. *Peyrilhe*, it is said, prescribed indiscriminately, the concrete and liquid ammoniac. This was the ground work of his practice, and what he added to the alkali, is merely incidental or collateral.

Hence it must be admitted that there is a close analogy between the doctrine and practice of Messrs. *Peyrilhe* and *Besnard*. Still it does not follow that the latter had copied the former. There is nevertheless, one very remarkable dif-

ference between them. *Peyrilhe*, exhausted perhaps by the attacks which the publication of his book had drawn upon him, or possibly undeceived as to the properties he had ascribed to his remedy, after some years gave up all further trials and resistance: while *Besnard*, more and more convinced of the efficacy of his prescription, to which he added opium, at the time when it was alledged that that substance would of itself work a cure, persevered in his opinion, and made all possible exertions to propagate it. He does not ask any person to credit him, on his mere declaration. He boldly appeals to experience and to facts; and solicits the judgment of the wise and the enlightened on the case. He has not protected himself by any patent or monopoly; but has published his mode of cure for the benefit of mankind, like a citizen of the world.

In justice to our own country, I must mention that the alkaline practice has been successfully pursued since the year 1798, in New York, for the cure of venereal ulcers, and other symptoms. The memoirs and statements contained in the first six volumes of the Medical Repository, will satisfy those who wish information on this subject, with what neatness and benefit, they have been employed in the great Hospital there.

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Williamson on Scrofula—in continuation.

AFTER a consumption has formed, or even when the lungs are much predisposed, if it be of an inflammatory nature, recourse should be had to depleting remedies and to an abstemious diet; the former reduce the action of the system by depriving it of a part of its excitement, and the latter decreasing the quantity of chyle, necessarily furnishes less excitement. By these means inflammatory tumors sometimes terminate by resolution. If a suppuration should supervene,

we ought to be careful not to pursue depletion too far, for no abscess can form healthy pus, if the tone of the interested solids are too much reduced, nor will an abscess heal if it do not form healthy pus. This is an obvious fact in external abscesses, and it can be no less true in those of the lungs and other internal parts of the system. If the consumption be of a scrofulous nature, a contrary mode of treatment is requisite.

The scrofula is a disease of debility, and any thing that debilitates the general system, or parts affected, increases the disease. When scrofulous tumours terminate in suppuration, the pus is generally bad, and is not to be improved but by increasing the tone of the solids; this is to be done by proper diet and strengthening medicines. In external scrofulous tumors which have suppurated, the bark has been proved to be very salutary; that it should be less so in those of the interior of the system, can only be accounted for from the peculiar situation of the disease, and not from any organ or viscus prohibiting its use. A healthy suppuration is as requisite in a scrofulous tumor as in a phlegmon, and it is as impossible for it to heal without it. In both cases it is desirable to prevent a suppuration, but if this cannot be done we are next to endeavor to produce a healthy one.

Exercise is very conducive to health, which cannot be supported without it. In diseases of the lungs when exercise becomes expedient it should be gentle. Sydenham speaks in the most exalted terms of riding on horseback in consumption. Coughing is always to be dreaded when severe, or of long continuance, whether there be an actual disease of the lungs, or merely a predisposition to one. If the person thus affected be of a scrofulous habit, he cannot be too solicitous about his fate. In cases of this nature, every effort should be used to prevent coughing. I have tried almost every remedy that has been hitherto recommended to allay and prevent coughing in patients of a scrofulous habit, who have been laboring under this affection, but have found none so

successful as a composition of gum opii. G. assafœtida et Tart. Ant. This composition has an admirable effect, especially if the nervous system participate in the disease.

Emetics have lately been recommended in consumption. The public fame of some writers is so great that their wildest theories are received as sacred by the great herd of readers, and the most implicit confidence placed in every thing they say. although their doctrines be indelibly stamped with error. I should have supposed that intuitive conviction against this practice would have been sufficient to have arrested it in its forming state, had man been deprived of the faculty of reasoning. That neither intuition nor reason have been sufficient to withstand an error of so much magnitude is to me incomprehensible. After taking a view of the structure of the lungs, the circulation of the blood, and the effect of an emetic on the lungs, we would suppose him an ignorant physician indeed who would prescribe an emetic in the consumption or hemoptysis. From the information we now possess we can not draw this inference, although we are fully persuaded that it is a bad practice, for we find men of reading and knowledge, and who have an extensive practice, complying with this theory: But

> "Knowledge and wisdom, far from being one,
> Have oft-times no connexion. Knowledge dwells
> In heads replete with thoughts of other men;
> Wisdom in minds attentive to their own."

In my history of the Influenza of 1807.* I have been led into a view of the use of emetics in that disease; the observations there made are equally applicable here. When writing that history I expected to have taken a more extensive view of the use of emetics in the disease now in question than my present engagements will admit of. I shall, however, make some additional remarks.

* Medical Repository—3d Hexade, 1st vol.

Important cases might be given from ancient and modern authors in confirmation of the injurious effects of emetics in consumption. Tulpius mentions a physician whose lungs had long been diseased, who expired soon after taking an antimonial; he attributes the death of the patient to the effect of the emetic. I have myself known several cases of hemoptœ where much injury has been done by emetics; and I felicitate myself on never having been accessary to such a practice.

June, 1809—At this period I have under my care a patient who confirms the truth of the above sentiments. This patient is a delicate female who has for several years been predisposed to pulmonary affections, and who has more than once laboured under a severe disease of the lungs. A few weeks since she contracted a violent catarrh, attended with a troublesome cough, pain in the breast, spitting of blood, &c. She was bled, had a blister applied to the breast, which was kept open for some time; antimonials, demulcents, diluents, &c. were exhibited. In a few days the spitting of blood ceased and the other pneumonic symptoms began to give way. About this period the warm weather commenced, and she was attacked with a bilious fever. The puking was considerable and attended with much exertion. The consequence was a return of the hemoptœ, with the other pulmonary symytoms which continued for several weeks. When the bilious attack took place we had to alter our mode of treatment and prescribe for this disease, which was soon subdued. Afterwards we recurred to our former remedies which had finally the desired effect.

I am not disposed to make these strictures on the use of emetics in diseases of the lungs in general; although I believe there are but few cases where they can be administered with advantage, if with safety; but I can not conceive them safe where there has been hemoptysis or where we have any reason to apprehend a disease of this nature.

Diet.—Much might be said under this head, but I shall confine my remarks to the article of milk.

Milk is very easy of digestion and at the same time very nourishing, and must therefore be salutary in consumption. That of a healthy woman is preferable when attainable. I knew a lady who was so much reduced by a fever that her life was despaired of by her physicians; she craved the milk of a woman, it was procured her, and she recovered. Van Swieten says "a young lady of high quality has used human milk for a year and more, with so good effect, that the cough, the purulent spitting, weakness, and emaciation, have quite ceased and she is alive in health and vigour." That milk will support adults for a long time, even when no other nourishment is taken has been proved. "A Roman lady of birth was accused of a capital crime before the Pretor. She was condemned to death and delivered to the triumvir, who ordered her to be carried to prison, and there to be strangled according to custom. The tears of the criminal influenced so powerfully the jailor, charged with the execution, that he resolved to let her perish with want and famine in the prison, rather than take away her life. He did more—he permitted her daughter to visit her, but took care that she did not take her any thing to eat. Some time elapsed and the jailor was surprised that his prisoner lived so long without taking nourishment. He observed her daughter with more attention. What a sight, he saw this generous daughter suckling her unfortunate mother to prolong the dreadful death to which she had been decreed by her humane jailor.

This heavenly act of filial love was rewarded by the magnanimous Romans in the most honorable manner. The mother and daughter were ordered to be supported the remainder of their days at the public expense, and a temple was directed to be built near the prison, dedicated to piety!

All the distinguished ancients eulogized milk as one of the best dietetic articles. Aretœus says, "milk is pleasant to the taste, easy to be drank, affording a substantial nourishment,

and more familiar from child-hood, to the body than any other food: besides its colour is agreeable to the sight; it causes no irritation to the organs of respiration; it lubricates the throat, and makes the expectoration of phlegm easy; it keeps the bowels lax; it is a pleasant remedy for ulcers, and more mild and salutary than any other. If any one drink a great deal of milk he will need no other food." Many of our most distinguished modern authors have also past the highest eulogies on milk. Among these I know of none who has treated this subject more interestingly than Tourtelle.

As this is a very interesting subject, and one with which the public are not sufficiently acquainted, permit me to give you my translation of Tourtelle on Milk.

"Milk is the first aliment of man and of the greater part of animals; it is the most proper food in the first age, and especially that of the mother. It is an animal emulsion, a white liquor, of a sweet taste, which is formed of three distinct parts, viz. of the serum or whey, of the cheese and butter. The serum is the vehicle of the other two substances, it contains a sweet substance called the sugar of milk, which is, as 't were, a sugar in its forming state. It also contains a calcarious phosphat which is more abundant in it, under similar circumstances, than in the other animal humors. It is the phosphat of lime that forms the case of the bones; this is the reason nature has placed it so abundantly in milk, for the growth and development of the bones, in the early periods of life. Cheese is a real albumen, and butter a concrete fixed oil; the concretion of which, and the separation from the other parts of the milk, by simple motion, are owing to the absorption and to the fixation of the oxygen of the atmosphere, during the formation of cream.

The proportions of serum cheese and butter are nearly the same, in the milk of the cow and that of the goat; that of the latter however contains a little more cheese than the

former; and that of the former a little more butter than the latter. The milk of the goat has a twelvth or thirteenth more of the sugar of milk than that of the cow. Their properties are nearly the same; however, that of the goat is not so relaxing and is a greater tonic, it sometimes passes off slowly and even constipates the bowels. The milk of sheep contains a fourth more of cheese, twice as much butter, and a third less sugar; its cheese is more tenacious, and consequently the milk not so easy to digest. That of the ass contains a third more of the sugar of milk than that of the cow, and but very little cream from which butter can be made. Spielman was able to obtain from a pound of asses milk, but a dram and a half of cheese; it was very nice. These three substances separate spontaneously by standing, but no coagulum can separate this milk, which is not so fat and rich, in cheese, as that of women. Spielman obtained from two pounds of woman's milk an ounce and an half of cream, which gave six drams of butter and half an ounce of very agreeable cheese. Mare's milk has the greatest affinity with asses; but what is very important to remark, is, that the proportion as well as the medical properties of milk, are owing to the actual state of the animal, to the nature of the food with which it is nourished, and to the passions it experiences.

Milk is an aliment adapted to the digestive forces of the new born child; it is the most proper for it, in consequence of the facility with which it is digested. That of the mother is the most proper, and no other answers fully as a substitute: In fact, the best nourishment for a child is that with which it commences its existence, and to which nature has gradually given the most proper preparation for its state, and for the development of its organs. Suckling has its limits, and it is as improper to suckle a child too short a time as too long. In general, suckling should not be discontinued in less than seven months, nor continued longer than twelve or fifteen, and the custom of not giving the breast more than nine months, is formed from experienced observation. Suckling

continued beyond a year or fifteen months, promotes or augments a disposition to the rickets, especially when dentition is performed slowly and with difficulty. It is not proper to give to children during the three or four first months any other nourisment than milk. It is not until the end of this period that other diet should be added, and when it is the quantity should be gradually augmented until the period of weaning. By adopting this plan we are not obliged to make great changes,and it is a real advantage, seeing that great changes generally occasion obvious, and sometimes fatal derangements in the system. There are signs by which we judge of the good and bad qualities of milk. Galen judged it by the taste, smell and appearance: he supposed that to be good which was of a sweet taste and without odour, or of an agreeable smell: he required it to be white, regular, and of a tolerable consistence. When we examine a small drop of it, it ought to preserve its round form; he considered that pernicious which was too thick or too thin, irregular, and especially that of which the taste was either bitterish or saltish. Primrose tried it by soaking a white linen in it, which he afterwards dried; and proscribed, with reason, that which communicated to the linen any colour whatever, except that which is natural to the milk.

When milk does not possess the natural qualities, it digests badly, and occasions acidity, colic, diarrhæa, convulsions, mesenteric obstructions, hectic, &c. When the bad qualities of the milk cannot be corrected by regimen, it is necessary to choose another nurse. Acidity and the other affections of which I have first spoken, are also frequently owing to the excessive use of vegetables, and soups; and at other times to the extreme weakness of the digestive organs. In the first case the nourishment of the child should be changed; in the second, which is generally known by finding the milk coagulated in the stools; the child should have some general tonic, and especially the watery infusion of rhubarb. Absorbent earths have been recommended by

physicians, as an efficacious remedy in the acidities to which children are subject; but experience and observation have not justified this practice. On the contrary they have taught us that the use of these substances generally increase the disease, and give rise to more serious accidents.

The milk of ruminating animals, which contains the most cheese, may, to a certain degree, supply the use of that of the mother, but it is never so salutary. It is only in cases where the child cannot be nourished without being exposed to danger, or where the milk is not proper for it, that the child should be weaned. In these instances, I prefer the milk of quadrupeds to that of a mercenary nurse; there are too many risks to be hazarded with the most of these women, who are compelled by a sordid interest or want, to renounce the quality of a mother, without speaking of the pernicious passions to which many of them give up, of their bad conduct, a want of care, and of many other similar causes which have a great influence on the children whom they nurse. How many women are there who transmit with their milk, the diseases and vices with which they are infested, and who entail on children a painful and miserable existence if death does not relieve them in early infancy?

Milk is a salutary aliment prepared by the hands of nature, and as it were, half digested. It is salutary, especially to persons whose digestive organs are very weak, as well as in all cases of exhaustion, in most cases of phthysic, cough, and of marasmus. A disposition to these diseases, and even the progress of time, have sometimes been arrested by a milk diet and proper exercise without any other remedies. The exercise should be proportioned to the strength of the patient. There are however cases of the same diseases where this aliment is not proper, as has been already well observed by Hippocrates: "Lac dare capite dolentibus, malum; malum vero etiam febricitantibus et quibus hypochondria elevata sunt murmurantia, et siticulosis; malum autem et quibus dejectiones biliosæ et quæ in acutis sunt febribus, et quibus copiosi sanguinis facta est ejectio. Convenit vero tabidis non

admodum valde febricitantibus lac dare, et in febribus longis et languidis, nullo ex supra dictis signis præsente, et præter rationem quidem extenuatis. (Aph. 64, sec. v.)*

Milk is improper in most of the cases mentioned in this aphorism, because they are accompanied with, or produced by the presence of gastric or intestinal saburra; consequently these substances, by altering the milk, augments the focus of irritation and corruption, and the disease is aggravated. For this reason Hippocrates also says: impura corpora, quo magis nutriveris, eo magis lœdes. (Aph. 10, sec. ii.)†

Milk is not proper, according to Hippocrates, to very thin persons, because the digestive juices have not a sufficient energy, and the powers of the stomach are insufficient to digest it; hence it acquires pernicious qualities, and gives place to nidorous, or acid saburra, to the colic and to a diarrhæa, which shortens their lives. A high fever also renders the use of milk improper, because the labour of digestion cannot then be properly performed, hence the milk is badly digested and becomes a new morbid stimulus.

The best manner of taking milk in the cases of which I have just spoken, is to suck the animal, or to swallow the milk just as it comes from the udder, or to warm it to the degree of heat which it has when first milked. But the two first are preferable, because the milk then still retains its flavour, and is, in this state, much the most restorative. It should never be boiled, for it then becomes much more difficult to digest, and loses its flavour.

* "To give milk to those affected with head-ache, is improper; nor is it adapted to the feverish, flatulent and thirsty; nor does it suit the bilious diarrhæa which occurs in acute fevers, where a great quantity of blood is evacuated. But it is proper for the consumptive, who have little fever, and it is salutary in slow, low fevers, continued beyond their ordinary period, when not contra-indicated by any of the preceding symptoms."

† The more food that is given when the humours are unconcocted, the greater is the injury committed."

The nourisment and constitution of the animal which furnishes the milk, are not objects of indifference: they have the greatest influence on its qualities, and merit the first attention. The milk of cows when in heat coagulates very difficultly. There are also many vegetables which eaten to a certain quantity by these animals, change the nature of the milk, and of its constituent parts. The shells of peas produce this effect. Milk preserves the colour, odour, taste, and most of the other properties of the aliments with which the animal is nourished. Saffron gives it a yellow colour, and madder a red: Thyme communicates its odour, and garlic its taste to it: Absynthium renders it bitter, &c. It were desirable then that the animal should only use plants which are proper in the species of disease for the cure of which milk is employed. There is no doubt but that by attending to these principles, we should succeed more certainly in curing and preventing diseases to which we ordinarily oppose but feeble arms, because these precautions are not taken in the use of this article of diet.

As to the constitution of the animal, it should be strong and robust; and should be of a suitable age, and in good health.

The milk of young animals is too thin, that of old ones too thick. The milk of an animal in heat is not good, nor that of an animal near the term of delivery, nor which has been lately delivered. The milk is better in the month of *Florial* and during summer than in winter: It is too thick in this last season.

Although milk is in general a very salutary aliment, its use alone, would not be sufficiently nourishing to strong and robust men, and to those who are habitually engaged in laborious work, it would not sufficiently support their energies, and would soon throw them into a state of debility and pernicious exhaustion. There are also, persons who do not digest milk, or who digest it badly, and to whom it is consequently injurious; in the first case, it produces constipation, and in the second, a sense of heaviness in the stomach, colic,

and diarrhæa. To remedy the constipation, it has been recommended, to combine with the use of milk, that of mild laxatives or injections, and when it causes pain and diarrha, to mix it with water, and to take at the same time, some gentle tonic. I have seen, in this case, good effects from rhubarb in powder, which I prescribed twice a day previous to taking the milk. When these means are ineffectual, and the milk continues not to digest easily, it is absolutely necessary to give up the use of it.

Butter possesses the same qualities that the other fixed oils do: it is nourishing. but like these, it is not easily digested by weak stomachs. The habitual use of butter relaxes the primæ viæ, throws them into atony, and promotes abdominal herniæ. Butter, as well as the fixed oils, turns rancid by age, and becomes bitter. In this state, it should be rejected from use. Cream is only butter mixed with a certain quantity of cheese: it is more easily digested than butter, but it sours in stomachs that abound with acids, and it may then produce very serious effects.

The *caseous* substance, or cheese, is the principle nutritive part of milk; it is very analagous to gluten, of which it possesses nearly all the properties. Cheese made of milk deprived of its cream, is very nourishing, but not very soluble in the gastric juice. It is only proper for robust persons. It is a strong and healing aliment, but nourishing. Caseus robustus est et œstuosus et alit. (Hipp. de diæta, lib. 2nd.) That which is made of the entire milk is not less nourishing, and it is more easily digested; it is still more so when the cream is added, for the particles of the latter interposed between those of the cheese, diminish its coherence. That made of cream alone is not so nourishing, but digests easily.

Cheese is used, not only when it is fresh, but also when it has gone through some degree of putrefactive fermentation. In this last state it is more or less acid and stimulant, according to the degree of putrefaction which it has experienced. When this is advanced it promotes the development of the germs which certain insects have deposited in it. Some

persons roast the cheese previous to eating it, but this process renders it more difficult to digest. In general, cheese should only be used in small quantities.

Caseus ille bonus, quem dat avara manus.

In this manner it aids digestion by its putrefactive quality, or, rather by its promoting the solution of the aliments in the gastric juice. Cheese that is too old communicates in the primæ viæ, the leven of putridy. That which is too much pressed converts the aliment into acidity. Those made of sheep's and goat's milk digest more easily than that of cows. It is with the milk of sheep that *Ionches'* white cheese is made, and the cheese of *Raquefort*. That made of milk which has not been boiled is the best. Lastly, the cheese of fresh cream is cooling and digests very easily.

Serum or whey is the least nourishing part of milk; besides its sugar, which appears to be the basis of the *lactic*, *sacho-lactic*, and *acetous acids*, which modern chemists have obtained from whey, and some salts which it also holds in solution; It also contains a portion of butter and cheese, that may be separated by clarifying it with the white of an egg or fish glue. It is only used as a medicine, and not as an aliment.

The Tartars and Kalmuchs, from time immemorial have used a wine, and a species of very strong brandy, which they obtain from the milk of the mare, by means of simple agitation. The agitation is sufficient to make the milk pass through the vinous fermentation. Nicholas Oseretskowsky, of St. Petersburg, has made many experiments on this subject, of which the principal results are: 1st. Skimmed milk does not undergo the vinous fermentation, neither alone nor with a ferment. 2nd. Milk agitated in a close vessel contracts this fermentation, and is converted into wine. 3d. Lastly, Milk fermented by the effect of heat, loses its alcohol, and is changed into vinegar."

Extract from a report made to the class of phisico-mathematical sciences, on the 9th of Sept. 1811, by a committee, composed of Messrs. Hallé, Humboldt and Percy, on a memorial of Mr. Legallois, M. D. concerning the principle of action of the heart, and the seat of that principle. Translated from the Paris Moniteur by J. D. Synott, M. M. & C. F. M. and Professor of Belles Lettres in St. Mary's college.

M. LEGALLOIS has undertaken to prove, in the memoir which he presented to the class, that the principle of the action of the heart resides in the spinal marrow.

The committee, in the first place, give a succinct account of the divers systems suggested since the discovery of the circulation to explain the action of the heart. The several systems of Descartes, Sylvius, Delebre and Borelli, and that of Willis, as revised and corrected by Boerrhaave, the systems of Stahl, Ens and Stachelin, are noticed by them in their turn. But the one which Haller founded on irritability has particularly attracted their attention. As this last has consigned all the others to oblivion, it has given rise to lively discussions which have been continued to the present day. These disputes proceed from this fact; that Haller's system, as well as those that preceded it, are not sufficiently satisfactory to account for all the phenomena.

In taking a general view (say the committee) of the different systems invented before Haller, we may remark, that the nervous power, in one way or other, has always been considered as one of the essential conditions to produce the motions of the heart, and the seat of this power has constantly and uniformly been placed in the brain.

The cardiac nerves had of course a determinate use in all these systems, and it was easy to conceive how the heart was subjected to the influence of the passions; but they could not explain why the circulation continues in acephalous animals, nor why, in experimenting on animals, the interruption of all communication between the brain and the heart, does not stop the action of the latter organ. Since the days of

Haller, irritability has been the basis of every system: in considering this property as essential to the muscular fibre, and independent of the nervous power, the circulation in acephalous animals and the different phenomena observed in the experiments above alluded to, present no difficulty; but the use of the nerves of the heart, and the influence of the passions on that organ became inexplicable. The necessity of obviating these difficulties has split the advocates of irritability into two sects. On one side the zealous defenders of pure irritability have called to their assistance the most inconsistent hypotheses, and all their exertions tend only to prove how very difficult it is to defend the cause they have embraced. Their opponents have introduced the nervous power with irritability, and consider the latter as one of the functions of the former; but then they were necessitated to admit, either with regard to the seat, or the mode of existence of this nervous power, certain conditions, which as they themselves confess, are far from being demonstrated, on which they do not agree among themselves, and which, when applied to the action of the heart, do not entirely remove the old difficulties, or else give rise to new ones.

It is easy to perceive whence it is, that so little progress has been made in this long and important controversy. If we examine all that has been said on the subject since the time of Haller, we shall find nearly the same facts, the same experiments, and the same reasonings advanced by either party. The only new experiments are the application of Galvanism to stimulate the cardiac nerves, and even these are new only in appearance, as in the days of Haller electricity was employed for the same purpose and with the same results. It is evident that we have nothing more to hope for the advancement of this interesting part of physiology by following the track already beaten by so many celebrated men, for nearly sixty years. New paths must then be opened, and new methods discovered for the investigation of nature: above all, that degree of precision and strict logical reasoning must be

introduced into physiological researches, which has been already employed with such success in the other departments of natural philosophy. This is what M. Legallois has endeavoured to effect.

It was not, at first, the object of M. Legallois to search into the causes of the movements of the heart. He was attached to the theory of Haller, until experiments, undertaken with quite a different view, led him to conclude that it was still a mystery to him how the nervous power, if it interfered at all, was interested in aiding the functions of the heart.

That we may give a clear view of his investigations, we will relate on what occasion, and by what chain of facts and reasoning he was induced to engage in this research.

A particular case of midwifery gave him, some years ago, a wish to know how long a full grown fœtus may live without breathing, counting from the moment when, from any cause whatever, it has ceased to have any communication with the mother. This question, so very curious in itself, and so interesting to the practice of midwifery and medicine, had scarcely been glanced at by authors. M. Legallois undertook to solve it by direct experiments. And in order that the solution might extend to as great a number of cases as possible, he placed the fœtus of different animals in such different situations as might resemble the principle accidents that may happen to the human fœtus whilst its communication with the mother is intercepted. Among these accidents there is one which but too often takes place; this is separating the head from the body in the delivery of a *footling* case. The author wished to know what became of the fœtus in this case, whether it died at the moment of decapitation, and of what death it died? He discovered that the trunk remains living, and that, by preventing hemorrhage by passing ligatures around the vessels of the neck, it does not die except in the same space of time, and with the same phenomena, as if, without being separated from the head, respiration had been completely intercepted. And what evidently

proved to him that the decollated animal is only in a state of asphyxy, is, that he could at will prolong its existence by supplying respiration by means of the inflation of the lungs.

M. Legallois concludes from these facts, that decapitation does no more than arrest the inspiratory motions, and that consequently the principle of all these motions is in the brain, but that the principle of the life of the trunk resides in the trunk itself. Searching afterwards for the immediate seat of each of these principles, he found that the principle of the inspiratory motions resides in that part of the *medulla ablongata*, that gives origin to the nerves of the eighth pain, and that that of the life of the trunk has its source in the *medula spinalis*. It is not by the whole of the spinal marrow that each part of the body is animated, but only by that portion from which it receives its nerves: so that by destroying a portion of the spinal marrow, we deprive of life those parts only of the body that correspond with this portion. Besides, if we intercept the circulation of the blood in a portion of the spinal marrow, life languishes and soon becomes entirely extinct in all the parts that receive their nerves from that portion of marrow. There are then two means of destroying life in this or that part of an animal body; one by destroying the marrow from which the part receives its nerves, the other, by intercepting the circulation of the blood therein.

The result of the above was, that the maintenance of life in any part of the body essentially depended on two conditions; namely, the integrity of the corresponding portion of the spinal marrow, and the circulation of the blood: and consequently, that it was possible to continue life in any part of an animal as long as these two conditions could be made to subsist; that one might, for example, continue life to the anterior parts alone, after having deprived the hinder parts of life, by the destruction of the spinal marrow that corresponds with them, or to the hinder parts, after having, in like manner, killed the anterior parts.

M. Legallois wished to know if his experiments would

confirm these consequences, and if it were in reality possible to enable this or that portion of an animal to live, independently of the other parts.

He first submitted to experiment a rabbit twenty days old; in this animal he destroyed all the lumbar portion of the spinal marrow. This operation not immediately interfering with the rest of the marrow, and the circulation, according to Haller, not being necessarily affected by it, there was sufficient room to expect, in reasoning from preceding experiments, that the animal would survive a considerable length of time; but in less than two minutes respiration was suspended, and in less than four minutes it no longer gave signs of life. The same experiment frequently repeated was always attended with the same results, without the possibility of preventing them by any means whatsoever, and it was invariably true that a rabbit twenty days old could not survive the destruction of its lumbar marrow. This was the more surprizing as rabbits of that age can continue to live after decapitation, that is, after losing the whole of the brain. This is the fact that M. Legallois could not reconcile with his preceding experiments, and which has led him to discover that the principle of the powers of the heart resides in the spinal marrow.

He first of all assured himself that the destruction of either the dorsal or cervical portion of the marrow was mortal to rabbits of twenty days old, as also of the lumbar portion, and even in less than two minutes. He next discovered that the same experiments repeated on rabbits of different ages did not produce the same effects. In general, the destruction of the lumbar marrow is not suddenly mortal to these animals before the age of ten days; many even of fifteen days survived it. With rabbits above twenty days the effect is the same as with those of twenty days. Very young rabbits may continue to live after the destruction of either the dorsal or the cervical marrow, but a much shorter time, and in fewer cases when the latter is destroyed, than the former. Those that

have passed the age of fifteen days can survive the destruction of either the one or the other.

In all these partial injuries, even when death speedily ensues, it is never instantaneous, except in such parts as receive their nerves from the portion of marrow destroyed. It does not take place in the rest of the body until after a certain time; but this time is determinate, and no means can prolong it. This time, which is the same for animals of the same age and species, is longer in proportion as the animals are younger. For example when the cervical marrow is destroyed in rabbits, life is extinguished on the instant through all the neck, but it continues in the head, which we may ascertain from the spasmodic motions of the mouth. Life remains also in the posterior parts, from the shoulders downwards, as may be perceived by the sensations and voluntary motions they exhibit. On the day of birth, the motions of the mouth continue about twenty minutes, those of the rest of the body fifteen minutes; at the age of fifteen days, the spasmodic gaspings do not last longer than three minutes, nor are the motions of the body perceptible longer than two minutes and an half. In fine, at the age of thirty days all motions cease within a minute or a minute and an half. After the destruction of the dorsal marrow, it is the breast and not the neck that is struck with death; in the rest of the body, the same phenomena and duration may be observed. If the three portions of the marrow be simultaneously destroyed, the gapings, (the only signs of life that then are manifest,) have, in different ages, the duration above mentioned.

The author, who had so often practised decapitation on rabbits of different ages, had constantly remarked, that the head, when separated from the body, continued to gape, and that during a time proportioned to the age of each animal. Now this space of time was the same as after the destruction of the spinal marrow. But it is evident that after decapitation there is no longer any circulation in the head, and that the spasmodic gapings which take place in this case, do not

continue longer than life subsists in the brain after the circulation has totally ceased.

This was what first indicated to M. Legallois, that when the partial destruction of the spinal marrow extinguished life in the rest of the body, it was by suddenly arresting the circulation. To make himself certain of this, he cut out, at the base of the large vessels, the heart of rabbits from the age of five or six days to a month; and having attentively remarked the duration of the different signs of life, counting from the moment when the circulation was arrested by the above method, he found that the spaces of time were exactly the same with those he had observed after the destruction of the spinal marrow. He might have considered this correspondence of time as sufficient to decide the question. But he wished to determine to a certainty, whether the circulation is really intercepted at the very moment when the spinal marrow is destroyed. The absence of hemorrhage, and the vacuity of the arteries were the most evident signs he could have of the fact, and he found that in effect, the carotids are empty immediately after this operation, and no blood was yielded on the amputation of the members, even when it was performed close to the body, and before life is extinct in parts the marrow of which has not been destroyed. In a word, all the signs that serve to give a knowledge of the state of the circulation proved to him that whenever the destruction of any portion of the spinal marrow suddenly causes death in the rest of the body, it is by arresting that function. This last effect takes place, not because the action of the heart ceases on a sudden, but because it loses all its force, so as not to be able to impel the blood into the carotids.

The result of all this is, that it is from the spinal marrow that the heart derives the principle of its powers, and from the whole of this marrow, since the destruction of any one of the three portions can, according to the age arrest the circulation, and the destroying of the three portions arrests it in all ages. Another result is, that each portion of this marrow

has on life two modes of action very distinct from each other. One, by which it essentially constitutes life in all the parts that receive their nerves from it; the other, by which it serves to maintain life in all the parts of the body, by contributing to furnish all the organs that receive twigs from the great sympathetic, and especially the heart, with the principle of life and power, necessary for their functions.

It is plain then, that in order to enable the fore or hinder parts of an animal to live singly, (after having caused death in the other parts of the body by destroying the corresponding marrow,) it is necessary to be able to prevent this destruction from arresting the circulation. To effect this it is sufficient to diminish by ligatures passed round the arteries, the extent of the parts to which the heart has to distribute blood. We have seen, for instance, that the destruction of the lumbar marrow is immediately mortal for rabbits that have attained or passed the age of twenty days; but they would not die of this operation, if, before we destroy the marrow, we were to commence by tying the abdominal aorta between the celiac and anterior mesenteric arteries.

The application of this principle to other parts of the body leads to a circumstance very singular in appearance; it is, that to be able to support life in rabbits of a certain age, after having destroyed the cervical marrow, we must begin by cutting off the head. They die inevitably, if without beheading them, we destroy this marrow. This fact ceases to surprise, when we remark that by decapitation we remove the entire head from the dominion of the circulation, and that by this means the heart having need of less force to continue its functions, we may weaken it by the destruction of the cervical marrow, without its ceasing to perform them. It is also easy to conceive that every other operation capable of suspending or considerably lessening the circulation in a certain extent of the body of an animal, must produce a similar effect, and afford an equal power of attacking without any

immediately fatal effects, a portion of the spinal marrow, the destruction of which would have been mortal without that preliminary.

This is what is obtained by the effect itself of destruction of the marrow. This destruction has two effects on the circulation: By the one it weakens the general circulation, by depriving the heart of the contingent powers it received from the marrow destroyed; by the other, without entirely arresting the circulation in the dead parts, it there diminishes it to a very great degree, so as to be to a certain point, equivalent to ligatures on the arteries of the parts.

But this effect does not plainly appear for many minutes after the destruction of the marrow. Thence it happens that the destruction of a first portion of the spinal marrow affords the means of destroying a second, this of a third, and so on. For instance, when, in beheading a rabbit of any age whatever, we are enabled to destroy the cervical marrow, the destruction of this marrow will, in a certain number of minutes, enable us to destroy the quarter of the dorsal marrow, and by continuing to operate thus at intervals on equal lengths of this same marrow, we at last succeed in destroying it entirely without arresting the circulation, which is then kept up by the lumbar marrow only.

We may gather from what we have said, that in rabbits any portion of the spinal marrow furnishes the heart with force sufficient to keep up the circulation in all the parts that correspond with this portion, and that consequently, if we were to cut a rabbit transversely, into several pieces, each piece might be made to live although insolated, if the lungs and heart, necessary to the formation and circulation of the arterial blood, could be included in the piece. But these can only form a part of the thorax; we might therefore very easily support life in the thorax alone, after having separated it from the rest of the body, and prevented hemorrhage by proper ligatures, and this would be the case in rabbits of thirty days and upwards.

Such are the principle results of M. Legallois's investigations. These results, which have a mutual dependance on each other, are founded on direct experiments, performed with a degree of accuracy hitherto unknown in physiology. The author, say the committee, has repeated them in our presence; we have spent three sittings, each of several hours, in examining them, and to avoid all precipitation, as well as to give ourselves time to weigh all the facts at leisure, we have suffered the interval of a week to pass between each session.

All the experiments which were repeated before the committee are ranked, in the report, under two heads. One of which contains those that go to prove that the *primum mobile* of all the inspiratory motions resides in that part of the medulla ablongata which gives origin to the nerves of the eighth pair. The other contains those whose object is to shew that the principle of the powers of the heart is in the spinal marrow. As these details, however interesting, are too long to be inserted here, we shall only say that the author has made similar experiments on warm blooded and cold blooded animals, and it is astonishing to observe the uniformity which experiments made on animals so different, offer in the results, even with regard to the principle of the inspiratory motions. It is well known, that it was particularly after experiments made on frogs that the followers of Haller, and among others Fontana had maintained that the motions of the heart were independant of the nervous power. But experiments repeated with the greatest care on the same animals before the committee, demonstrated at once the inacuracy of this opinion, and of the experiments on which it was founded. The gentlemen of the committee conclude the recital of the experiments repeated before them, by declaring that they completely confirmed all the results above mentioned, and by adopting all the conclusions which the author has deduced from them.

They then continue in these words:

That we may here confine ourselves to the principle points we will say what we consider as demonstrated.

1st. That the principle of all inspiratory motions has its seat in that part of the *medulla ablongata* which gives origin to the nerves of the eighth pair.

2d. That the principle which animates each part of the body resides in that part of the spinal marrow whence arise the nerves of that part.

3d. That it is, in like manner the spinal marrow which furnishes the principle of life and action; but in this the spinal marrow is throughout concerned, and not a particular portion of it.

4th. That the great sympathetic has its origin in the spinal marrow and that the particular function of this nerve is to bring all the parts to which it is distributed under the immediate influence of all the nervous power.

These results easily solve all the difficulties that have sprung up since the days of Haller, on the causes of the motions of the heart. The principle of these were, to explain for what purpose the heart receives nerves, why it is submitted to the empire of the passions; and why the circulation continues in acephalous animals and such as have been beheaded. No explication hitherto given was sufficient to reconcile all these points. But at present we can easily conceive why the heart receives nerves; and why it is so evidently subject to the empire of the passions, since it is animated by the whole of the spinal marrow. We may also see that when the circulation continues in acephalous animals, &c. it is because the motions of the heart do not depend on the brain, or at most depend on it but secondarily.

We ought to remark that this last point, on which M. Legallois has thrown so much light, presents nothing but confusion and error in the authors of the Hallerian, as well as of the new school. None of them have distinguished the motions of the heart that take place after decapitation, from those that are observed after the excision of this organ, or after the destruction of the spinal marrow.

The committee next proceed to testify that M. Legallois

is indebted to his own industry and zeal for his success in these researches. They then conclude in these terms:

In a word, it appears to us that we may say of the different authors who have considered the matters that M. Legallois has treated, what M. La Place so justly said on a like occasion. "One may find among them many facts, but these are almost always blended with numberless errors, and the discovery of the truth belongs only to him who separates it from the mass, and establishes it on a solid basis, by calculation or by observations."

The opinion of your committee is, that M. Legallois by the service he has done to physiology, merits all the encouragement the class can afford him; and that his researches are of more importance to this branch of knowledge than any that have been made since the days of Haller.

(Signed,)

DE HUMBOLT,
HALLE,
PERCY.

The class approves of the report, and admits the conclusions drawn in it.

The class moreover decrees that the report shall be printed in the records of the class, and that a committee shall consult with M. Legallois with regard to the expense incurred by him for the experiments already made, and the means of continuing them.

G. CUVIER,
Perpetual Secretary.

Hints on the subject of Animal Secretions.

BY EVERARD HOME, ESQ. F. R. S.

Communicated by the Society for the improvement of Animal Chymistry.*

From Philosophical Transactions for 1809, *part II.*

THE brilliant discoveries of Mr. Davy on the powers of electricity in producing chymical changes, suggested to me the idea that the animal secretions may be produced by the same means.

To prosecute this inquiry with every advantage, requires a knowledge of anatomy, physiology, and chymistry, rarely to be met with in the same person. I have therefore availed myself of the assistance of the different members of this society; the object of which is the improvement of animal

* Dr. Wollaston's observations inserted in the Philosophical Magazine, were published after this paper had been laid before the society.

I was led to the present investigation, while preparing my lectures on the Hunterian Museum, in which the secretions in different animals are to be considered. In September last, I engaged Mr. William Brande to assist me in prosecuting the inquiry. In November, I communicated my opinions to Sir Joseph Banks, and stated that I should bring them forward in my lectures. At that time Dr. Young's Syllabus was not published, and Dr. Wollaston's opinions were unknown to me.

Dr. Berzelius, professor of chymistry at Stockholm, published a work on Animal Chymistry, in the year 1806, in the Swedish language, in which he states, in several places, that he believes the secretions in animals to depend upon the nerves, although he is unable to explain how the effect is produced. In proof of this opinion, the following experiment is adduced.

"Trace all the nerves leading to any secretory organ in a living animal, and divide them, being careful to injure the blood vessels and the structure of the organ itself, as little as may be: notwithstanding the continued circulation of the blood, the organ will as

chymistry. Their intimate acquaintance with these branches of science renders them peculiarly fitted for such an undertaking.

It is one of the most important subjects to which Mr. Davy's discoveries can be applied, and he has given it the consideration it deserves.

The Voltaic battery is met with in the torpedo and electrical eel: and although it is given only as a means of catching their prey, and defending themselves, and therefore not immediately applicable to the present inquiry, yet it furnishes two important facts: one, that a Voltaic battery can be formed in a living animal; the other, that nerves are essentially necessary for its management; for, in these fish, the nerves connected with the electrical organs exceed those that go to all the other parts of the fish in the proportion of twenty to one. The nerves are made up of an infinite number of small fibres, a structure so different from that of the electric organ, that they are evidently not fitted to form a Voltaic battery of high power; but their structure appears to Mr. Davy to adapt them to receive and preserve a small electrical power.

That the nerves arranged with muscles, so as to form a Voltaic battery, have a power of accumulating and communicating electricity, is proved by the well-known experiment of taking the two hind legs of a vivacious frog, immediately after they are cut off, laying bare the crural nerves, applying one of these to the exposed muscles of the other limb, and then when the circle is completed, by raising the other crural

little secrete its usual fluid, as an eye deprived of its nerve can see, or a muscle whose nerve has been divided can move. We may therefore easily conceive, that any trifling alteration in the nerves of a gland may materially affect its secretion, the supply of blood being in every way perfect."

He says, the agency of the nerves in secretion has generally been disregarded, because our attention is only called to their secret mode of acting when we discover the insufficiency of all other explanation. Dr. Berzelius's work was shown to me by Mr. Davy while this paper was in the press.

nerve with a glass rod and touching the muscle of the limb to which it does not belong, the muscles of both are excited to contractions.

There are several circumstances in the structure of the nerves, and their arrangements in animal bodies, which do not appear at all applicable to the purposes of common sensation, and whose uses have not even been devised. Among these are the plexuses in the branches of the par vagum which go to the lungs, and in the nerves which go to the limbs. The ganglions, which connect the nerves belonging to the viscera with those that supply the voluntary muscles, and the course of the nerves of the viscera which keep up a connexion among themselves in so many different ways.

The organs of secretion are principally made up of arteries and veins; but there is nothing in the different modes in which these vessels ramify, that can in any way account for the changes in the blood, out of which the secretions arise. These organs are also abundantly supplied with nerves.

With a view to determine how far any changes could be produced in the blood by electricity, at all similar to secretion, Mr. W. Brande, who has begun his career in animal chymistry with so much success, made the following experiments, in the suggestion of which Mr. Davy afforded him every assistance.

EXPERIMENT I. Middle of January, 1809.

The conductors from twenty-four four-inch double plates of copper and zinc, charged with a very weak solution of muriatic acid, were immersed in four ounces of blood, immediately on its having been withdrawn from a vein in the arm. The temperature of the blood was kept up at 100° during the experiment. The apparatus was so constructed as to admit of the products at the negative and positive wires being separately collected and examined. When the electrization had been carried on for a quarter of an hour, all action seemed to have ceased. The blood which had surrounded the negative wire was of a deep red colour and extremely

alkaline; that surrounding the positive wire was slightly acid, and of a brighter hue.

In this experiment, the coagulation of the blood was not materially affected by the electrical power alluded to.

EXPERIMENT II. 8th of February, 1809.

Finding it necessary to submit perfectly fluid blood to the action of electricity, the following experiment was undertaken with a view of keeping it the longest possible time in that state.

A deer having been pithed, the abdomen was immediately opened into, and a length of about four inches of a large vein in the meso-colon was detached from the neighbouring parts. Two small platina wires, connected in the usual way with forty three-inch double plates, were inserted into this detached portion of vein, and secured by ligatures, having their points at a distance of about one inch from each other. The communication with the battery was kept up for one quarter of an hour; a third ligature was then tied in the centre of the detached vein, in order to cut off the connexion between the positive and negative ends. On removing the portion of the vein included by the ligatures, and containing the conductors, it was found that the gaseous products had forced out nearly the whole of the blood, at the part through which the wires were inserted; alkaline and acid matter were readily detected, but no new product could be discovered.

Finding the coagulation of the blood an insurmountable obstacle to the long-continued electrical action, the serum only was employed in the following experiments.

EXPERIMENT III. 10th of March, 1809.

The conductors from one hundred and twenty four-inch double plates, highly charged, were brought within two inches of each other, in some recent serum of blood, obtained free from the colouring matter, by carefully pouring it off from the coagulum. Coagulated albumen was rapidly separated at the negative pole, and alkaline matter evolved;

at the positive pole, a small quantity of albumen was gradually deposited, and litmus paper indicated the presence of acid. These are the effects produced by a high electrical power upon serum.

EXPERIMENT IV. 14th of April, 1809.

Was undertaken to ascertain the effect of a low power: a battery was employed, consisting of twelve four-inch double plates of copper and iron. In this case, there was at first no appearance of coagulation at either pole, in five minutes, the positive wire became covered with a film of albumen, and in fifteen minutes a filament of about a quarter of an inch in length was seen floating in the fluid, and adhering to the same wire.

EXPERIMENT V. 6th of May, 1809.

Two small platina cups, connected by a large quantity of cotton well washed, and each containing one ounce of serum, were rendered positive and negative, by thirty double three-inch plates very *weakly* charged. The process was continued during twenty-four hours. This power had not been sufficient to produce coagulation at the negative pole. On examining the fluid in the negative cup, it was found to consist principally of an alkaline solution of albumen.

The fluid in the positive cup was rather turbid, it reddened litmus, and was slightly acid to the taste. On standing, it deposited a few flakes of albumen. When evaporated, it afforded saline matter, with excess of acid, (super salts.)

By these experiments it is ascertained, that a low negative power of electricity separates from the serum of the blood an alkaline solution of albumen; that a low positive power separates albumen with acid, and the salts of the blood. That with one degree of power, albumen is separated in a solid form; with a less degree, it is separated in a fluid form.

From these facts, the following queries are proposed:

1st. That such decomposition of the blood by electricity, may be as near an approach to secretion as could be expect-

ed to be produced by the artificial means at present in our power.

2d. That a weaker power of electricity, than any that can be readily kept up by art, may be capable of separating from the blood, the different parts of which it is composed, and forming new combinations of the parts so separated.

3d. That the structure of the nerves may fit them to have a low electrical power, which can be employed for that purpose; and as such low powers are not influenced by imperfect conductors, as animal fluids, the nerves will not be robbed of their electricity by the surrounding parts.

4th. That the discovery of an electrical power, which can separate albumen from the blood in a fluid state, and another that separates it in a solid state, may explain the mode in which different animal solids and fluids may be produced, since, according to Mr. Hatchett's experiments, albumen is the principal material of which animal bodies are composed.

5th. That the nerves of the torpedo may not only keep the electric organ under the command of the will, but charge the battery, by secreting the fluid between the plates, that is necessary for its activity.

6th. As albumen becomes visibly coagulated, by the effect produced from twelve four-inch double plates of copper and iron, a power much too low to affect even the most delicate electrometer, may not this be occasionally employed with advantage as a chymical test of electricity, whilst the production of acid and alkali, affected by still inferior degrees of electricity to those required for the coagulation of albumen, may likewise be regarded as auxiliary tests on such occasions?

If these facts and observations appear to the society to throw any light upon the principle of secretion, it may be an advantage to medical science, that they should be laid before the public, as hints for future inquiry.

Kelso on the use of Cotton in Burns.

SIR,

SINCE facts are the only true tests of the efficacy of any remedy, I beg leave to submit the following case for your consideration. In perusing the first number of the Lycæum, my attention was attracted by Dr. Dallam's paper on the use of cotton in scalds and burns. Having long wished for a remedy that would procure relief from the severity of pain in such cases, it appeared to me that the cotton was the remedy so much to be desired. Never having found a remedy that would satisfy me on this point, and having frequently been disappointed in the spt. terebinth. (the celebrated cure of Dr. Kentish) which generally was found to aggravate the pain on its first application; I determined to resort to the use of the cotton as soon as a suitable case should present itself. As our patients flatter themselves with the expectation of immediate relief on our arrival, it appeared to me that even the temporary mitigation of pain would be no small acquisition.

On the 16th of April, 1812, I was requested to visit a child in the country, between two and three years old, that had been accidentally scalded, by its mother having spilt a pot of boiling water over a considerable part of its body.—When I arrived at the house I found the child in great pain, and upon examination, the water had extended from the os occipitis to the os calcis; it had extended over the deltoid, latissimus dorsi and glutæi muscles; the whole put on a high scarlet appearance; a slight vesication arose on the tip of the left shoulder and on the left glutæus muscle. I lost no time in applying the cotton as directed by Dr. Dallam; and in about twenty or thirty minutes was agreeably surprised to find the little sufferer fall into a sound sleep. I remained in the house an hour after the child began to sleep, and then was compelled by other business to leave it, with particular orders to continue the cotton. The next day it was not in my power

to visit my little patient, but I was informed that it awoke perfectly free from pain, and returned to its usual habits of playfulness. A slight covering was still kept to the parts to keep them from rubbing or getting any way injured, as the skin remained tender. On the next day I paid it a second visit, and upon examination I found that the inflammation had entirely subsided, the scarlet colour having disappeared. Nothing now seemed to be necessary except to keep the part covered from the air.

It appears to me, from the extent and great degree of inflammation first produced, that had it not been prevented by the cotton, one general vesication would have been the consequence. As the application in such cases is a new remedy, it would be highly gratifying to me should this case, together with Dr. Dallam's observations, tend to excite others to similar trials.

D'r sir, your ob't humble serv't

JOS. KELSO.

Dr. Potter. *Harrisburgh, 4th May*, 1812.

.....

Interesting particulars concerning a new and undescribed species of nettle (urtica) which is capable of being manufactured into yarn, thread, cordage and cloth. In a letter from Samuel L. Mitchell, M. D. dated Washington, Jan. 27, 1812, to the editor.

Governor Tomkins to Dr. Mitchill.

NEW-YORK, Nov. 16th, 1811.

SIR,

MR. CHARLES WHITLOW, of this island, has discovered a weed which he thinks will be a complete substitute for hemp and flax. He avers that it can be raised in larger quantity upon the acre than either of them, that it is not any species of hemp now known, and that it has no botanical

name. I enclose you a few inches of the top of a stem before breaking, and the flax of a whole stem, broken with the fingers, and a short cut from a skein of thread spun of the flaxen part before it was either rotted or bleached.

Dr. C. W. Eddy to Samuel L. Mitchill.

NEW-YORK, Nov. 21st, 1811.

I duly received your note of the 19th, this morning, and being unfortunate in not having a knowledge of the plant from which the hempen material is procured, I took a ride his afternoon to see Mr. Whitlow, from whom I learned that it was as yet a secret, and that it was unknown to Walter, Michaux, Persoon and all others who have at any time written on or described the plants of this country; accordingly I desisted from all farther enquiries on that point. He however, shewed me some large skeins of *thread*, and also a considerable quantity of tow, made of it, adding that the woman who spun it, declared that should it be properly rotted and bleached she could make it produce thread of a quality one half finer. I was also informed by Mr. Whitlow that the thread weighed at the rate of *ten dollars per pound.*

Opinion in reply.

WASHINGTON, Dec. 20th, 1811.

This plant is a native of New-York. It is a hardy perennial. It is believed by Muhlenberg to be a species not hitherto described by any botanist. It belongs to the genus of URTICA, which abounds in plants having stringy and fibrous coats, applicable to the manufacture of thread, ropes and even cloth. The *nettle* now under consideration is proposed to be named in honour of the discoverer, *Urtica Whitlawi.* Its character, though sufficiently distinct, is however such as to place it nearer to the *Urtica Divaricata* and the *U. Canadensis* than to any others. If this plant will afford a good crop of *cortical* or *fibrous* matter, its durability will give it a preference to such annuals as flax and hemp, by saving the yearly expense of tillage and seed.

The following is its Botanical character.

CAULE—5 angulari, simplici, orgyali, urente.

FOLIIS—Alternis, cordato, ovatis, acutis, serratis, trinerviis, punctatis, petiolatis, supremis oppositis.

STIPULA—Bifida.

PANICULIS--Pedunculatis, axillaribus, dichotomiis, hirsutis, petiolo longioribus masculis, et terminalibus fœmineis.

CAPSULA—Orbiculari, compressa, mucronata, proxima *divaricatae* et *Canadensi.*

RADIX.—Perennis.*

For which the scientific world is indebted to Muhlenberg.

Certificates of its value.

At the request of his excellency governor Tomkins and his honour the mayor of New-York, we, the subscribers, manufacturers of linen, flax and cotton, met Mr. Charles Whitlow, botanical collector, for the purpose of examining a perennial plant, discovered by him, which he exhibited to us in its various stages. Having particularly examined the same, we are unanimously of opinion that it is far superior to any hemp or flax we have seen, either in Europe or America, in the quantity it produces from a single stem. The superior strength, beauty and fineness of its texture, and the experiment that has been made proves that it will produce from twenty to

* For the use of those who may not readily comprehend Latin technical terms, we subjoin an English translation:

STEM.—Simple five angled, irritating by pungent stimuli.

LEAVES.-Heart-oval, acute, serrated, three nerved, dotted, placed on alternate footstalks below, though opposite above.

STIPULES.—Bicleft.

FLOWERS.---Axillary pannicles on bifurcated pubescent stems longer in the males than the footstalks of the leaves. Female flowers terminal.

CAPSULE.--Orbicular, compressed, daggered, resembling that of the divaricated and Canada species.

ROOT.---Perennial.---*(Editor.)*

twenty-five per cent more from the hackle than any flax or hemp known to us. And we are fully persuaded that it will become a great benefit as a staple article to any country that will encourage its cultivation.

Dated, New-York, this 14th Nov. 1811.

JOHN CRITCHTON,
F. STEVENSON,
ROB'T MITCHELL,
ROB'T THOMSON.
ZAC. PEACOCK.

[*True Copy.*]

The plant above mentioned, with the flax, tow and thread produced from the same, having been examined by us, we are of opinion that its cultivation will be highly beneficial to any country that will encourage it.

THO'S C. BUTLER,
CORNELIUS BENSCOWFER,
BURNELL BROWN.

[*True Copy.*]

New-York, Nov. 16th, 1811.

We have seen and examined a plant in the hands of Charles Whitlow, which so far as we are capable of judging from viewing it in the different stages of its manufacture, as presented to us by Mr. Whitlow, appears well calculated to answer all the purposes of hemp and flax, and merits cultivation assiduously, as it promises to be highly beneficial to every country, which will encourage its cultivation and manufacture.

JOHN KEMP, } Professors of Co-
P. WILSON, } lumbia college.

Attested, FRED. PURSH.

Observations on a new plant of the hemp and flax species; lately found by Mr. Charles Whitlow.

This article appears to be particularly valuable on account of the harl being inclosed in the body of the substance, and

not like hemp or flax, merely covering a pithy or wooden substance.

As the pithy or wooden substance in hemp or flax occupies at least from three-fourths to seven-eighths of its body, I think myself qualified to assert that this plant will produce from three to four times more than the present species of flax or hemp now cultivated. The plant appears to be a species of flax. Its productive growth recommends it as likely to be a valuable article of agriculture. The harl is really strong, it spins easy and works readily and is well adapted for machinery. On the whole I think it a discovery which entitles Mr. Whitlow to a compensation for his trouble and reward for his investigation.*

JOHN DALTON.

New-York, 15th Nov. 1811.

* The discovery of this article and the economical purposes to which it may be applied, ought to act as a strong inducement to the lovers of knowledge, to cultivate an acquaintance with natural history. Our country abounds in animals, vegetables and minerals that may be brought into use and render life more comfortable, as well as increase the ratio of population. Millions of acres lie waste around us, only because we are without a vegetable to suit the habit or capacity of the soil. The barren mountain may by future discoveries be made a garden; and the noisome marsh that now evolves only miasmata deleterious to animal life, may be made to blossom as the fruitful valley.

The hemp can scarcely find a spot in our Atlantic States to satisfy its rapacious demands, while plants perhaps of equal value, may be found to flourish on our stony fields and grounds, rendered by long use incapable of the usual crops.

By a knowledge of botany we identify plants, give intelligent descriptions of them, and are led to form the strongest conjectures both as to their medicinal and economical uses. Thus we see that from a slight resemblance in character Linnæus has denominated one of the species of urtica *cannabina*, and that Jussieu has placed the *canuabis* and *urtica* in the same class and order of plants.—*Editor.*

Hopkins on the poisonous effects of Strammonium.

SIR,

AGREEABLY to promise I send you some account of the poisonous effects of Strammonium or Thornapple. The following case came under my notice about fifteen years ago, and has taught me a lesson that has been useful to me:

A girl about eight years old, remarkable for the enjoyment of good health, had left the house perfectly well: on her return she threw herself on the bed and presently became stupid, to all appearance insensible, and finally delirious. Upon examination I fond the pulse irregular, and an extraordinary dilatation of the pupils was remarkable. As the family could give me no account of the cause of the symptoms, I supposed the disease to have arisen from worms; and immediately exhibited ten grains of Calomel and a gill of Castor Oil which were swallowed with great difficulty, on account of the great want of sensibility and an aversion to all exertion or motion. I had the patient then immersed in warm water, and having waited two hours in vain, for the operation of the medicine, I gave her ten grains of Calomel, with two of Jalap, and two of Gamboge. To these medicine I added a table spoon full of Castor oil every half hour, and followed the doses by stimulating injections to quicken the operation of the rest.

In about five hours the medicine began to operate; and to our surprise more than a table spoon full of the seeds of the Strammonium were discharged at the first evacuation, and smaller portions at several successive discharges. They came away just as they had been chewed, partly masticated, but unaltered by the action of the gastric juice. The child was immediately cured, and the case brought to my mind the truth of the old adage, that "the cause being known the disease is half cured;" for my ignorance of the cause of the malady lost to me the benefit of

a more speedy cure, the exhibition of a brisk emetic. As it happened, the effects of the active medicine and my having remained with the patient saved her life. My principal reason for wishing to record this case is, that should such other cases occur (as no doubt they frequently do) other practitioners may avail themselves of my misconstruction of the cause and use a more prompt and energetic mode of relief.

About a year after I attended the above case I was sent for in haste by a family that was in great consternation on account of three of their children. They had gone out of the house in good health, and when they returned the father and mother were astonished at the uncommon appearance of their eyes, the pupils being widely dilated. As soon as I saw them and witnessed their behaviour and appearance, I had no difficulty in ascribing their complaint to the right cause. On interrogating the children they readily acknowledged that they had been employed in plucking the leaves of the Strammonium, and that after bruising them they had rubbed them over each others faces.

Since I drew up the above cases, I was requested to visit a child about three years old. It had plucked a pod of the Thornapple and had eaten about two tea spoons full of the seeds. It soon became quite stupid and insensible. The family unsuspicious of danger, neglected to procure medical assistance till the morning after the afternoon on which it had swallowed the seeds; and the child died about an hour after I reached the house.

Let it not be inferred from these deleterious effects of the Jamestown weed that it may not be, when properly prepared and judiciously employed, a valuable acquisition to the Materia Medica. The best medicines in too large doses are poisons and the most virulent poison in proper doses is often the best medicine. We have so many proofs of the virtues of this medicine both externally and internally, that it would be superfluous to dilate on the subject.

With best wishes your friend.

RICHARD HOPKINS.

Observations on Albumen, and some other Animal Fluids; with Remarks on their analysis by electrochymical decomposition.

BY WILLIAM BRANDE, F. R. S.

Communicated by the Society for the Improvement of Animal Chymistry.*

I. *Observations on Mucus, and on the composition of liquid Albumen.*

THE results obtained from the chymical analysis of the intervertebral fluid of the spalus maximus, an account of which is annexed to Mr. Home's paper "On the nature of the intervertebral substance in fish and quadrupeds,"† led me to undertake a series of experiments on mucus, in order to examine the properties of that secretion in its pure state, and to ascertain how far it might be capable of conversion into modifications of gelatine and albumen.

1. Saliva was the first source of mucus to which I directed my attention.

In order to separate the albumen, which Dr. Bostock's analysis has shown it to contain,‡ it was agitated for a short time with an equal quantity of pure water; the solution was then boiled and filtered. I considered the clear fluid, which had passed the filter, as a solution of nearly pure mucus; but found, on applying to it the tests of nitrate of silver, and acetate of lead, that it still contained a very considerable proportion of saline matter. The precipitate consisted of muriate and phosphate of silver and lead, in combination with a little animal matter, the odour of which was perceptible on exposing it to heat after it had been washed and dried.

One thousand grains of saliva afforded, by careful evaporation in a water bath, a residuum weighing one hundred and eighty grains, from which twenty grains of saline matter, consisting of phosphate of lime and muriate of soda, were obtained by incineration.

* Philosophical transactions for 1809, p. 373.

† Philos. Trans. 1809; or Journal, vol. XXV. p. 214.

‡ Nicholson's Journal, vol. XIV. p. 149.

2. The mucus from the trachea, and that of the oyster were next examined; but here the proportion of saline matter was greater than in the former case, although no traces of albumen could be detected by the usual tests of heat, alcohol and acids.

Finding, therefore, that the re-agents employed to detect mucus* act principally upon the salts which it contains, and not merely upon the secretion itself, it became an object of some importance to find out a method of depriving it of its saline ingredients, by such means as should not affect the mucus. Decomposition by electricity immediately occurred to me, as the most likely means of attaining the object I had in view.

For this purpose I procured three glass cups, each capable of holding rather more than a measured half ounce of water; one of these was filled with a mixture of equal parts of saliva and pure water; this was connected with the other two, containing pure water, by filaments of moistened cotton. The water in one of the cups was rendered positive, that in the other negative, by a Voltaic battery of one hundred and twenty four inch double plates, charged with a solution of nitro-muriatic acid, in the proportion of one part of the mixed acid to thirty parts of water.† By continuing this process, I hoped to decompose the saline ingredients of the saliva, to collect the acid matter in the positive, and the alkaline matter in the negative cup, and thus to leave the mucus and albumen in the centre vessel (free from the salts which they contain in their natural state,) and to have separated them by boiling distilled water, which would then have afforded a solution of pure mucus.

* Nitrate of silver and acetate of lead. Vide Thomson's system of Chymistry, vol. V. p. 500, third edition; and Nicholson's Journal XI...251.

† It was conceived that this electrical power, though sufficient for the decomposition of the salts, would not materially affect the animal matter.

When the action of the battery had been continued for about ten minutes, a considerable quantity of a white substance surrounded, and adhered to the cotton on the negative side of the circuit, whereas on the positive side no such effect had taken place.

I could not at first account for this appearance, conceiving that, if it depended on the coagulation of albumen held in solution in the saliva, it would have taken place at the positive pole, in consequence of the acid there separated.

To ascertain this point, an experiment was made on the albumen of an egg.

When the conductors from the same battery were brought within two inches of each other in this fluid, an immediate and rapid coagulation took place at the negative wire, while only a thin film of albumen collected at the positive wire, where its appearance was readily accounted for, by the separation of a little acid, which, reacting on the albumen, would render it solid; but the cause of the abundant coagulation at the negative pole was not so obvious.

This result I mentioned to Mr. Davy, who immediately offered an explanation of it, by supposing the fluidity of albumen to depend upon the presence of alkaline matter, the separation of which, at the negative pole, would cause it to assume a solid form. I had only to follow up this idea, and shall proceed to state the principal experiments which were undertaken to establish so probable an opinion.*

1. When coagulated albumen, cut into small pieces, is boiled in distilled water, it imparts a viscidity to that fluid, showing that something is retained in solution.

Two hundred grains of the coagulated albumen of an egg were repeatedly washed and triturated in four ounces of distilled water, which was afterwards separated by a filter, and

* On referring afterwards to Dr. Thompson's system of Chymistry, (vol. V. p. 491) I find, that a very similar explanation of the coagulation of albumen has been offered by that author, which the following experiments will likewise confirm.

evaporated to about one fourth of its original bulk. It was then examined by the usual tests, and was found evidently alkaline; it converted the yellow of turmeric to a pale brown, and restored the blue color to litmus paper, reddened by vinegar; but it did not appear to effervesce on the addition of a diluted acid.

On evaporating this alkaline fluid to dryness, by a gentle heat, a viscid substance, soluble in water, was obtained. This solution was rendered slightly turbid by an acid; and by the application of electricity, from sixty four-inch double plates, a copious coagulation took place at the negative pole.

So that water, in which the coagulated white of egg has been boiled, is in fact an extremely dilute alkaline solution of albumen.

This enables us also to explain why albumen becomes coagulated simply by heat.

When the coagulated white of egg is cut into pieces, a small quantity of a brown viscous fluid gradually separates from it, as has been observed by Dr. Bostock in his paper on the primary animal fluids.* This I find to consist principally of an alkaline solution of albumen. It reddens turmeric, and coagulates abundantly on the application of negative electricity.

It appears, therefore, that the white of an egg, in its fluid state, is a compound of albumen with alkali and water; that when heat is applied to it, the affinities existing between these bodies are modified; that the alkali, before in chymical combination with the albumen, is transferred to the water, and that this separation causes the coagulation of the albumen: the aqueous alkaline solution, which is thus formed, reacts upon the coagulated albumen, of which it dissolves a small portion, and then appears in the form of the brown viscous fluid already noticed.

The coagulation of albumen by alcohol and by acids may be explained by a reference to the principles already laid down.

* Nicholson's Journal, vol. xi—246.

1. Five hundred grains of the white of egg were agitated with two ounces of pure alcohol; an immediate coagulation resulted, which was rendered more perfect by the application of a very gentle heat. The liquor was separated from the coagulum by filtration, and evaporated to half its bulk; when the usual tests were now applied, alkaline matter was abundantly indicated.

In this instance then, the albumen, in passing from the liquid to the solid state, gives its alkali to the alcohol.*

2. When acids are applied to albumen, these effect its coagulation from the same cause: they render it more rapidly and more perfectly solid, on account of their superior affinity for the alkali.

The following experiments were instituted with a view to ascertain the nature and quantity of the alkaline matter which exists in liquid albumen.

1. Five hundred grains of the liquid white of egg were mixed with two ounces of distilled water, and exposed for half an hour to a temperature of 212°. The fluid was then separated by a filter, and the coagulated albumen cut into small pieces, and repeatedly washed with boiling distilled water. The filtrated fluid was evaporated to half an ounce by measure; it had a saline taste, it was somewhat turbid, and slightly alkaline; on cooling, it gradually deposited a few flakes of albumen: it was electrified positively in a small glass cup, connected by washed cotton to another similar vessel containing a little distilled water, negatively electrified by one hundred four-inch plates, charged with a solution of nitro-muriatic acid of the same strength as that employed in a former experiment, fresh portions of water being occasionally added in order to compensate for the loss by its decomposition.

When the electrization had been carried on in this way for one hour the cups were removed, and their contents examined.

* When albumen is coagulated by alcohol, it does not become so perfectly solid as in most other instances, because the separation effected by the relative affinities is not so complete.

The fluid in the negatively electrified cup acted rapidly on turmeric, rendering it deep brown. On evaporation and subsequent exposure to a low red heat, it afforded a residuum weighing 5·5 grains, which had the properties of soda, in a state approaching to purity.

The positive cup contained a little coagulated albumen, and an acid which was principally, if not entirely the muriatic, was held in solution by the water: it gave a very copious precipitate with nitrate of silver, which became speedily black on exposure to light. When saturated with carbonate of soda, and evaporated, it afforded a salt in small cubic chrystals, from which the fumes of muriatic acid were developed by the action of the sulphuric.

This experiment shows, that, exclusive of soda in an uncombined state, fluid albumen contains some muriate of soda.* We learn from the experiments of Mr. Hatchett, that minute quantities of other saline bodies are likewise present.†

In the foregoing experiments, I had generally employed from sixty to three hundred four-inch double plates of copper and zinc, but in subsequent researches, made with a view of ascertaining the action of lower powers, the effects of which I shall afterwards relate, I find that a battery of twenty

* May not a submuriate of soda exist in fluid albumen?

† After the destructive distillation of coagulated, dry, semitransparent albumen, there remained "a spongy coal of very difficult incineration; as towards the end of the process it appeared vitrified, and glazed with a melted saline coat, which was, however, easily dissolved by water. The residuum was again exposed to a long continued red heat, and again treated with water, till, at length, a few scarcely visible particles remained, which, as far as such small quantity would permit to be ascertained, proved to be phosphate of lime. The portion dissolved by water (which was by much the most considerable) consisted principally of carbonate, mixed with a small quantity of phosphate of soda.

"Five hundred grains of dry albumen afforded 74·50 grains of coal, of which 11·25 were saline matter."

See "Chymical experiments on Zoophytes, with some observations on Component Parts of Membrane." Phil. Trans. 1800.

four three-inch double plates is sufficient to effect a perfect coagulation at the negative pole, even where the albumen is diluted with so large a quantity of water, as not to be detected by the usual tests.

II. *Observations on the Composition of some animal Fluids containing Albumen.*

Finding, from the experiments detailed in the preceding section, that albumen may exist in such states of combination, as not to be detected by the usual tests, but separable by electrical decomposition, I was induced to apply this mode of analysis to the examination of animal fluids in general.

1. *Saliva.*

When saliva is boiled in water, a few flakes of coagulated albumen are deposited; but this is by no means the whole quantity of albumen contained in the secretion, for on applying the test of negative electricity to the filtered fluid obtained after the separation of the albumen by heat, a copious coagulation and separation of alkali are produced at the negative pole. A large portion of albumen may therefore exist in a fluid, incapable of separation by heat, and in the present instance not to be detected even by acids, these re-agents producing no effect on the filtered solution just alluded to.

2. *Mucus of the Oyster.*

The solution of mucus obtained by agitating oysters in water exhibits to the usual tests no traces of albumen; but when acted upon by electricity from the Voltaic battery, a considerable and rapid coagulation takes place at the negative pole, by electrified wire.

3. *Mucus of the Trachea, &c.*

The other varieties of mucus, as from the trachea, the nose, &c. agree with the former, in affording abundance of albumen by electric decomposition; whereas scarcely any traces of this substance can be detected by the tests of acids, heat, or alcohol.

In these experiments, alkaline matter was always evolved at the negative, and acid at the positive wire. Minute researches, made with a view of ascertaining the nature of the alkaline and acid matter thus evolved, showed the former to consist of soda, with traces of lime; the latter of muriatic acid, with traces of phosphoric acid in the cases of saliva, and mucus of the trachea and the nose: the mucus of the oyster afforded only soda and muriatic acid.

On examining the proportions of alkali and acid, the former seemed always to predominate, although in the original fluids no traces of uncombined alkali (as in the white of egg) are to be detected.

These results lead to new ideas respecting the composition of mucus: Is it a peculiar combination of muriate of soda and albumen? or may it not be a compound of soda and albumen, in which the alkali is not separable by the usual modes of analysis, but which yields to the superior decomposing energy of electricity?

4. *Bile.*

An immediate coagulation took place in this secretion, at the negative conductor, the albumen being tinged throughout of a green colour, arising from the colouring matter at the same time separated.

The relative proportion of albumen, separable by electricity from different specimens of ox-bile, was found to be liable to considerable variation, so that a detailed analysis of this fluid cannot be generally depended upon. I have found the albumen in bile to vary in quantity from 0·5 to three per cent.; and it is somewhat remarkable, that where there is a small quantity of albumen, there likewise the proportion of the resinous matter of bile is relatively small.

The electrochymical decomposition of this fluid affords, beside the results just mentioned, a considerable quantity of soda at the negative pole; and at the positive pole, a mixture of muriatic and phosphoric acids.

5. *Milk.*

In this fluid, the separation of albuminous matter at the negative pole is equally evident, though not so rapid, as in most other cases. The conductors from sixty four-inch double plates, highly charged, and immersed within four inches of each other in three ounces of cow's milk, during one hour, produced the appearance of curds and whey, the principle part of the curd being collected in the neighbourhood of the negative wire, and but little at the positive wire. When this experiment was so conducted, as to collect the products in separate vessels, the predominating ingredients in the contents of the negative cup were soda and traces of lime; and in the positively electrified vessel, a mixture of muriatic and phosphoric acids.

After such decomposition of milk, the serum still affords sugar of milk.

6. *The Liquor of the Amnios.*

An opportunity having offered of examining this secretion, from the human subject, in its pure and fresh state, I shall mention the general results of its analysis.

The liquor of the amnios is almost perfectly transparent, but on exposure to air becomes gradually turbid, and deposits a white flaky matter. It renders tincture of violets green, and while perfectly fresh does not affect litmus; but sulphurated hydrogen is soon evolved from it, and then it slightly reddens litmus. When heated, it becomes turbid, and lets fall flakes of coagulated albumen. Acids render it slightly turbid from the same cause.

Alkalis produce no change, unless when added in considerable excess: the odour of ammonia is then perceptible.

Electrical analysis afforded albumen and soda at the negative pole, and muriatic acid at the positive pole. Hence we learn, that the liquor of the amnios has the properties of a dilute solution of liquid albumen.*

* The difference in the results of the analysis given in the text,

7. *Pus.*

In the pus of a healthy sore, coagulation took place at both poles; most abundantly, however, at the negative pole. A slight degree of putrefaction having commenced in the pus which was examined, I did not pay particular attention to the other products of the experiment.

In concluding this section, it may be proper to remark, that the decomposition of liquid albumen by Voltaic electricity takes place in different ways, according to the power employed. With a comparatively high electrical power, the coagulation goes on rapidly at the negative pole, and only very slowly at the positive pole; whereas, with an extremely low power, the coagulation is comparatively rapid at the positive surface, an alkaline solution of albumen surrounding the negative pole. Thus, when the conductors from twenty four four-inch double plates, highly charged, were brought within half an inch of each other, in a dilute solution of albumen (consisting of one part of albumen to six of water,) the coagulation was considerably more abundant at the negative than at the positive pole: but when the conductors were removed from each other to a distance of eight inches, or when they remained at half an inch, being connected with a battery of six four-inch double plates only, the coagulation was only perceptible at the positive pole, in consequence of the acid there collected. Hence we may infer, that a rapid abstraction of alkali is necessary to the perfect coagulation of albumen, since, in the cases above alluded to, the albumen remains in solution.

and that of VAUQUELIN and BUNIVA, most probably arises from the liquor of the amnios examined by those chymists not having been perfectly recent, and perhaps mixed with other secretions.---See *Annales de Chimie*, XXXIII. p. 279.

From the American Medical and Philosophical Register.

A Memoir upon the *Organs of Absorption* in Mammiferous Animals, read at the Institute, on the 7th of August, 1809, by M. Magendie, Doctor of Medicine of the Faculty of Paris, Professor of Physiology, &c. The experiments conducted by Doctors Magendie and Delile. Communicated to Dr. David Hosack, by A. R. Delile, M. D. of the Institute of Egypt, &c.

AMONG the facts which I had the honour to report to the class, in a memoir upon the upas tieuté, the nux vomica, and the bean of St. Ignace, there is one which appeared to me worthy of more particular attention; I mean the readiness with which those poisonous matters are absorbed and introduced into the sanguiferous system. It must be recollected, that it scarcely requires twenty seconds to convey these poisons from the peritoneal cavity to the spinal marrow.

The generally received ideas relative to the organs of absorption, do not admit of a doubt that the lymphatic vessels are the agents for conveying these poisons into the sanguiferous system. Thus, in the experiment where the poison was introduced into the middle of the thigh of an animal, there was but one way of explaining its absorption; it must necessarily be admitted that it was taken from the wound by the lymphatic vessels of the parts with which it was in contact; that after being absorbed it was carried by these vessels towards the glands of the groin; that after traversing these bodies, it was conveyed, still by the lymphatic vessels, to the thoracic duct; finally, that it was introduced into the sanguiferous system by the communications which the thoracic duct preserves with the subclavian veins, and principally with those of the left side.

Such ought to have been, and such, in fact, was our opinion, at the time of the publication of the memoir upon the strychnos. Nor were the experiments, of which I am about to give an account, undertaken with a view to discover new facts, but rather to add a degree of certainty to an explana-

tion already admitted; and our labours did not take a particular direction until a great number of facts obliged us to modify our view of this subject. But so rapid an absorption by vessels, whose principal characteristics are weakness and slowness of action; a poisonous substance that so quickly pervades the difficult and winding route of the lymphatic glands, without any alteration therein, were two circumstances that ought, perhaps, to have made us entertain some doubts of the correctness of the received explanation. This explanation, however, is given by so many respectable persons, and is supported by experiments so positive, that even now, when we have many facts to oppose it, we dare not say that it wants exactness, but only, that it is not admissible under every circumstance.

Previous to any detail of our experiments, it will not be useless to relate, in a few words, an opinion which for some time balanced the present prevailing belief relative to the organs of absorption.

This opinion, professed by Boerhaave, Haller, Méckel, Ruysch, Swammerdam, and others, was, that the sanguiferous veins, in common with the lymphatic vessels, possessed an absorbing power.

It is supported by different circumstances of structure, and by some physiological and pathological facts. A series of interesting experiments, undertaken and executed a few years since, at the veterinary school of Alfort, has also strengthened the probability of such absorbent property of the veins, but without producing entire conviction. It is well understood that an opinion established upon the physical structure of the organs, deduced from a sufficient and conclusive number of experiments, and supported by the names of Boerhaave, Haller, and Ruysch, ought not to be easily abandoned. Nor was less required than the anatomical discoveries of the last century, the correct experiments of Hunter and his brother, those of Cruikshank, Mascagni, Desgenettes, and others,

to establish the belief that the lymphatic vessels only possess the absorbent faculty.

I will further cite, in support of the general opinion, some very curious experiments lately made by M. Dupuytren.—This physiologist, who has kindly permitted me to report the principal results of his labours, tied the thoracic duct in several horses; some of them died in five or six days, others preserved every appearance of health. We know already by an experiment of Duverney, by some observations on the thoracic duct when obstructed, and above all, by the experiments of Landrin, that the thoracic dust may cease to convey the chyle into the subclavian vein without being followed by the death of the animal; it is true, we also know, that some animals died in consequence of a ligature round the duct; but we are entirely ignorant of the cause of this diversity in the results. M. Dupuytren, by his experiments, has found one very satisfactory. In the animals that died in five or six days, from the ligature round the thoracic duct, he always found it impossible to pass any injection from the inferior part of the duct into the subclavian vein; consequently, it is very probable, that the chyle ceased to be conveyed into the venous system immediately after the application of the ligature. On the contrary, in all the animals who have survived the application of the ligature, it has always been easy to make every kind of liquid pass from the inferior part of the duct to the subclavian veins, by means of the very numerous communications between these two points by the lymphatic vessels, placed alike in the posterior as in the anterior mediastinum.

I have personally assisted M. Dupuytren in opening a horse, the thoracic duct of which he had tied more than six weeks before, and I easily satisfied myself that there existed evident communications between the inferior portion of the duct and the subclavian veins, although this canal was entirely destroyed at the place of the ligature.

I now proceed to the experiments, which I made, for the most part, in concert with M. Delile, to determine, whether

the lymphatic vessels are really the only ones by which foreign substances enter into the venous system.

A circumstance that has always thrown some obscurity over the experiments upon absorption, is, the difficulty of demonstrating, with certainty, the passage and presence of absorbed matters, either in the lymphatic or sanguiferous vessels. We have not these inconveniences to fear in employing the upas, or the *nux vomica;* for it is known that two centigrammes of these substances produce effects too remarkable to be mistaken.

Would the ligature round the thoracic duct stop the passage of the poison in the sanguiferous system, and consequently, its effects upon the spinal marrow? This was the first question that we proposed to resolve.

I have applied in a dog a ligature to the thoracic duct, a little before its opening into the left subclavian vein; I afterwards introduced a solution of the *upas* into the cavity of the peritoneum. The effects of the poison were as prompt and marked as if the thoracic canal had not been tied. I have tried the same ligature in other animals; but instead of introducing the poison into the cavity of the peritoneum, I introduced it either into the pleura, or into the stomach, the intestines, or muscles of the thighs, &c. The effects have always been equally rapid and intense, as if the thoracic canal had been free.

Decisive conclusions cannot be drawn from these first essays, for we know that the thoracic canal is not the only point of communication between the lymphatic and venous systems. There is ordinarily on the right side, a second thoracic duct, almost as considerable as that on the left; the large lymphatic vessels often open themselves solitarily into the subclavian veins; and more frequently still the thoracic duct has several mouths into the vein where it terminates.

One of these circumstances was found to occur in the animals subjected to our experiments; and we must have recourse to other trials, from which other results, less equivocal, may be deduced.

Upon a dog who had eaten a large quantity of meat seven hours before, in order that his abdominal lymphatic vessels might be easily perceived, we made an incision through the parietes of the abdomen, and took from thence a coil of the small intestines, upon which we applied two ligatures about five inches apart from each other. The lymphatics, which had their origin in this turn of the intestine, were very white and very apparent from the chyle which they contained. Two ligatures were placed at the distance of one centimetre upon each of these lymphatics; we cut these vessels between the two ligatures; we took great care in this part of our experiment, and assured ourselves, by every possible means, that the curvature of the intestine, taken from the abdomen, had no further communication with the body by the lymphatic vessels. Five arteries and five mesenteric veins ended in the portion of the intestine comprised between the two ligatures; four of these arteries and four of the veins were tied and cut in the same manner as the lymphatics; the two extremities of the curvature of the intestine were cut and entirely separated from the rest of the small intestine. Thus we had a portion of the intestine about five inches in length, not communicating with the rest of the body but by one artery and one mesenteric vein; these two vessels were separated at the distance of four fingers breadth. We even raised the cellular substance, lest some lymphatic vessels might lie there concealed. We had nothing further, by which to obtain a positive result, than to inject a small quantity of the *upas tieute* into the cavity of the intestinal curvature. This was also done with proper precautions to prevent the escape of the injected liquor. The curvature of the intestine, enveloped in fine linen, was replaced in the abdomen; it was then precisely one o'clock. To our great astonishment, about six minutes after, the general effects of the poison diffused themselves with their usual intensity, and in such a manner that every thing proceeded as if the curvature of the intestine had been in its natural state.

The animal being dead, we examined the parts; no ligature

was displaced, nothing could make us suppose that the poison had passed into the abdominal cavity.

This experiment, repeated several times without any modification in the result, appeared to us most conclusive; it proves, at least, as far as can be proved in physiology, that the lacteal vessels are not the exclusive organs of intestinal absorption.

This kind of absorption, different from that of the lymphatic, may be peculiar to the intestines; it would be important to know if it could be discovered to exist in the other parts.

We separated from its body, the thigh of a dog, previously rendered comatose by opium, (in order to spare it the pain of a difficult experiment;) this separation was so made, that the thigh still communicated with the trunk by the *crural* artery and vein. We took, with regard to these two vessels, the same precautions as for the mesenteric vein and artery in the preceeding experiments; that is, we isolated them on an extent of four centimetres, and raised their cellular *coat*, least it should conceal some lymphatic vessels; we then inserted two grains of the poison in the foot, and waited the effects. They manifested themselves with as much promptitude and energy, as if the thigh had not been separated from the body; insomuch, that the first signs of the action of the upas appeared before the fourth minute, and the animal died before the tenth.

It may be objected, that notwithstanding all the precautions taken, the arterial and venous parietes still contained some lymphatics, and that these vessels were sufficient to give passage to the poison. It were easy to refute this objection.

I repeated upon another dog, the preceding experiments, with this modification, that I introduced into the crural artery a small quill, upon which I fixed this vessel by two ligatures; the artery was afterwards cut circularly between these two bands. I did the same to the crural vein; so that there was no longer any communication between the thigh

and rest of the body, unless by the arterial blood which comes to the thigh, and by the venous blood which returns to the trunk. The poison introduced into the foot produced its general effects in the ordinary time, that is, in about four minutes. It may be deduced, I believe, from these different experiments, that the lymphatic system is not, at least in certain cases, the exclusive route that foreign substances take to arrive at the venous system.

This new mode of absorption, much more direct than that by the lymphatics, presents the means of easily conceiving the rapidity with which the different deleterious and other matters are absorbed, as well as the rapidity with which they produce their effects upon the system.

But what are the organs that first absorb the poison from the parts where it has been introduced? Are they the radicles of the veins, or are they rather the capillary lymphatics, which, having immediate anastomosis with the sanguiferous capillaries or exhalents, would immediately diffuse the poison through the venous system?

The experiments which I have just related, joined to those which have been made on the same subject, appear to me totally insufficient to decide either of these questions; only, it ought to be remarked, that our experiments are strongly in favour of a direct absorption by the veins.

But it is a fact, rendered evident by the preceeding experiments, and upon which it is necessary to pause a moment, that the venous blood becomes charged with the poison, and that by the intervention or means of this blood, the poison produces its deleterious action upon the system. In fact, if in the experiments where I had separated the thigh from the trunk, we suspend the course of the venous blood, by compressing between two fingers the crural vein, we lessen, and even totally suspend, the production of the effects. The blood of an animal, in which the signs of action of the *upas* is developed, contains then, some portion of poisonous matter; indeed, it may be said to be really poisoned. It were curious and interesting to know, if this blood carried

into the circulatory system of a healthy animal, would produce effects similar to those it had upon the animal itself. At first sight, we are led to believe that this is extremely probable, even that it is certain. The following experiments will shew with what care we ought, in physiology, to distinguish that which is probable from that which is proved by experiment.

We passed the arterial blood of an animal, in which the tetanus caused by the upas was manifest, into the jugular vein of a healthy animal; the transfusion lasted nearly twenty minutes, so that the healthy animal received a very considerable quantity of poisoned blood, which at the first moment of the experiment, was of a red and vermillion colour, and which afterwards became violet and black, when the upas had produced asphyxia. There did not, however, appear any irritation of the spinal marrow, and the animal only experienced what happens in common transfusions, made with care. I mean, that it had, for some hours a very marked acceleration of the inspiratory and expiratory motions, as well as a very abundant pulmonary exhalation. Frequently repeated, this experiment has always produced the same result.

We were now certain, that the arterial blood of animals poisoned by the *upas tieute*, the *nux vomica*, or the *bean* of *St. Ignace*, was not susceptible of producing similar effects on other animals; it would not be, perhaps, the same with the venous blood. It may be presumed, that the respiratory action changed the nature of the poisonous substance; and this alteration might, to a certain degree, give the reason, why the transfusion of the arterial blood of animals poisoned by the strychnos, is not followed with bad effects.

This circumstance did not take place with the venous blood which returns from the part where the poison has been introduced. After the experiments reported in the memoir upon the upas, and in this, it is impossible to doubt that this blood does not transport the poison to the lungs. It is very probable that introduced into the circulatory system of another animal, it would produce effects similar to those which it

caused upon the animal on which the inoculation of the poison was made.

A small piece of wood covered with two grains of *upas tieute*, was stuck into the thick part of the left side of the snout of a dog. Three minutes after this introduction, we passed into the venous system of another dog, the blood of the jugular vein of the side where the introduction of the poison had been made. The transfusion commenced about one minute before the first signs of the upas; it did not cease till the death of the animal who experienced it. No appearance of irritation of the spinal marrow was perceived in the animal who received so great a quantity of poisoned blood.

Although these experiments were repeated several times, with variations in the mode of introducing the poison, we never could perceive in the healthy animals, who had suffered in the transfusions of poisoned blood, any thing which resembled the effects of the strychnos.

Results so positive, appear to us of a nature to warrant the conclusion, that the venous blood of animals poisoned by the *upas*, the *nux vomica*, and the *bean* of *St. Ignace*, is no more capable than the arterial blood, of producing upon another animal the effects which it will cause upon the animal from which it was taken.

If there still remained any doubts, they would be removed by the following experiment, which was repeated several times.

As in the experiments above related, we separated from the body the thigh of an animal, insolated as before the crural artery and vein; we introduced the poison into the separated foot, and transfused the blood of the crural vein into the jugular vein of a sound animal. The passage of the blood from one animal to the other, lasted more than ten minutes, a time more than sufficient for the production of the effects of the upas. But no sign of the action of this poison was perceived, either in the one or the other animal. The one preserved perfect health, the other died in a few days, in consequence of the amputation of the thigh, and from the loss of blood which was transfused.

It must not, however, be thought, that, in this experiment, the transfused blood, by some particular cause, had no deleterious properties, for the following experiment proves the contrary.

As in the preceeding experiment, I separated the thigh from the body; three minutes after introducing the poison into the foot, I passed the blood of the crural vein into the jugular vein of another animal; the transfusion was prolonged five minutes without producing any effects. I then stopped it, and disposed things in such a manner, that the blood of the crural vein should return to the animal to which it belonged. Almost instantly this animal exhibited evident signs of the action of strychnos on the spinal marrow.

From the different experiments reported in this memoir, we must, I think, conclude,

1st. That the lymphatic vessels are not always the route followed by foreign matters, to arrive at the sanguiferous system.

2nd. That the blood of animals, upon which the bitter strychnos have produced their deleterious effect, cannot produce any fatal effects upon other animals.

As to the explanation of this singular phenomenon, it would be, I think, premature to give it at present. In physiological science, we ought to be sparing of conjectures, and prodigal of facts.

MAGENDIE, D. M. P.

Note.—This memoir has received the approbation of the Institute.

Dr. Hall's Treatise on the Acetite of Lead.

DR. POTTER—SIR,

AT your request I send you some observations on the tonic powers of the Acetite of Lead, when exhibited internally.

In intermittents of obstinate character, and more particularly in those of protracted duration, I have used the Acet. Plumb. with almost uniform success; and in those cases where an uncommon delicacy of stomach has caused the bark to be rejected, this has produced no unpleasant effect, but has immediately arrested the progress of the disease. In numerous cases a few doses of the above preparation have been effectual when blisters and a variety of tonics had been unsuccessfully used. The dose in which I have generally given it, is to an adult about three grains, and less according to the age of the patient. In the intermission I have commonly directed it to be given and repeated every second or third hour, according to the danger to be apprehended from the return of a paroxysm. In many cases where the remissions were so imperfect as to render the use of the bark doubtful, the Acet. Plumb. was administered with success. It produced no dryness of skin, head-ache, or such symptoms as usually follow from exhibiting the cortex in a febrile state of the system.

In the treatment of the intermittent and remittent fevers of children I have found the above preparation of lead of peculiar utility.*

Its taste and quantity do not produce that aversion which is manifested by children to large and repeated doses of Chincona. I would by no means wish to recommend the article above spoken of in the treatment of intermittent fevers to the exclusion of those which have generally been in use among us, but as a powerful auxiliary to bark and ton-

* My friend and associate, Dr. G. E. Mitchell, informs me that he has also used this medicine with much success in the intermittents and remittents of children.

ics in general. In my own case during the autumn of 1810, from a peculiar irritability of stomach, the bark produced a distressing emesis: the gentian, columbo, and other medicines were equally injurious and the cerus. acet. completed the cure.

In those profuse and debilitating nocturnal perspirations which often succeed intermittents, no article of the Materia Medica has under my direction afforded such certain and immediate relief as the medicine of which we have been speaking. In those cases I generally direct it to be taken a short time previous to the hour of rest. From the success which has attended the use of it in the night sweats of intermittents, I have been led to exhibit it with a view to check the sweats of hectic fever. Although it did not as in the former cases so fully answer the intention, yet it generally afforded much relief, by checking in a greater or less degree those profuse and debilitating evacuations.

In no case which has fallen under my observation has the internal use of the cerus. acet. been productive of consequences in the least degree injurious to the constitution of the patient. Yet I would not recommend its exhibition in any case for a length of time, knowing that effects of an unpleasant nature *might* be the result. Such would also be the results from a protracted use of many other articles of the Materia Medica, which are used by all without hesitation. This does not furnish an argument for their rejection, but for the more cautious direction of them by the physician.

RICHARD W. HALL.

Baltimore, 15th March, 1812.

From the Edinburgh Review, for November, 1810.

A letter on the differences in the structure of Calculi, which arise from their being formed in different parts of the Urinary passages; and on the effects that are produced on them by the internal use of solvent medicines, from Mr. William Brande, to Everard Home, esq. F. R. S.—[From the Philosophical Transactions for 1808, Part II.]

Some observations on Mr. Brande's paper on Calculi. By Everard Home, esq. F. R. S.—(From the same volume.)

An account of a Calculus from the human bladder, of uncommon magnitude. By Sir James Earle, F. R. S. [From the Philos. Trans. for 1809, Part II.]

Observations on the effects of Magnesia in preventing an increased formation of Uric Acid; with some remarks on the composition of the Urine....Communicated by Mr. William T. Brande, F. R. S. to the Society for the improvement of Animal Chymistry, and by them to the Royal Society.....(From the Phil. Trans. for 1810, Part I.)

On Cystic Oxyde, a new species of Urinary Calculus. By William Hyde Wollaston, M. D. Sec. R. S.....(From the same work, Part II.

THESE curious and valuable papers relate to one of the most excruciating torments with which Divine Providence has, for wise purposes, decreed that the lot of man should be mingled, during his existence in this world. Indeed, were it not that custom teaches us to consider mental afflictions as, in the general, more severe than any bodily pains, (and, no doubt, to some natures, they are beyond all comparison, more unbearable,) we should be disposed to rank the dreadful complaint which we are about to treat of, as the worst of human maladies. To investigate the nature of the Stone, for the purpose of discovering solvents which might remove it, has accordingly been long considered as one of the noblest problems of practical chymistry, and among the best services which that science could render to the healing art. It may be remembered, that to this investigation we owe the brilliant discoveries of Dr. Black, which, by disclosing the na-

ture of fixed air, and of the alkaline earths, paved the way for all the modern improvements in chymical knowledge. Those great acquirements in speculative science were the compensations which he obtained for his disappointment in a search far more important and interesting to humanity: as the navigators of the fifteenth and sixteenth centuries discovered the new world, and explored its strange recesses, while occupied in the pursuit of an earthly paradise, or a fountain of health and youth. Even the legislature of this country took some part in those inquiries, and gave rewards to the discoverers of solvents. The most noted instance is that of Stephenson; and here the money was granted upon the faith of a cure *said* to be performed, the body not having been examined after death. We need scarcely add, that medicine thus patronized, and, as it were, privileged, has long since been completely exploded; and though, in its failure, the parliament resembled the celebrated philosopher whose name we have mentioned, we do not remember that its investigations were like his, rewarded by any other discoveries.

The hopelessness of the search for *solvents*, has lately turned the attention of the chymists and physiologists to another problem; and they have been occupied in ascertaining the process of calculous formations, with the view of administering such *preventive* medicines as may indispose the system to produce those concretions, or check their growth, without altering the tone of the constitution. The papers now before us throw considerable light on this important subject; and contain also a good deal of information, tending to elucidate the fallacies of the older doctrines respecting solvents. Indeed, did we not know the danger of trusting to medical reports of cures, and did we not recollect how regularly a series of cases, all quite apt and most nicely fitting the doctrines, form a part of every modern treatise on medical subjects, we should be tempted to consider a great part of the question solved with regard to the stone, and should

confidently look to the extirpation of that worst of maladies. Even moderated by such unpleasant recollections, as we own our hopes to be, we cannot look to the eminent names prefixed to these tracts without great respect; and we feel at least so much inclined to listen, that we shall make no farther apology for laying before our readers an account of what the papers have added to the stock of our knowledge on this subject.

In order to convey some idea of the greatest degree in which this disease has ever yet, we believe, been known to afflict its wretched victims, we shall begin with noticing the case narrated in Sir J. Earle's paper.

Sir Walter Ogilvie, a Scotch Baronet, received, at the age of twenty-three, a severe blow on the back, from the boom of a vessel; and the pelvis and lower extremities were in consequence paralyzed. He remained in bed for fourteen months, in an horizontal position; and, during the first two months, his urine was of necessity drawn off by a catheter. After this, he had somewhat recovered the use of his limbs, and could walk or ride, though with difficulty: his general state of health, too, was weak and precarious. About twenty years after the accident, he was afflicted with symptoms of stone; and upon examination, a calculus of considerable size was found to have been formed in the bladder. An operation was now recommended, but, unfortunately, delayed from time to time, although the patient's health constantly declined, and the irritations and pains in the bladder greatly increased. In this melancholy state he continued for eight years longer, when he became unable to make water in an upright position: he was obliged to place himself in an inverted posture, that the upper part of the bladder might become the lower; and as, even by this means, very small quantities of urine only could be voided, the irritation of endeavoring to make it became almost perpetual, and he was obliged to perform the above mentioned operation every ten minutes, with scarcely any relief. The principal remedies which he tried were *aqua*

calcis and *uva ursi*. He used opium also to alleviate the pain; and this, adding to a naturally costive habit, rendered aperient medicines frequently necessary, which, again, must have increased the irritations of the disease. At the age of fifty-three, thirty years after the accident, the spasms became so constant, and so violent, that he resolved to have the operation performed; and the paralysis of the lower parts being deemed no obstacle to it, he was conveyed by sea to London, where Sir James Earle and Mr. Cline consulted upon his truly lamentable case, about the end of July, 1808.

Upon examining the lower part of the belly, a tumor was perceived, so large, that sir James Earle doubted its being altogether caused by the stone; but, on sounding, the instrument would not enter the bladder; and further examination showed, that the whole cavity was nearly filled with calculus. It was clear, therefore, that this could only be extracted, if it should prove soft enough to admit of being broken, and thus brought away piecemeal; and it was deemed impossible to attempt the operation above the *os pubis*. The ordinary method was therefore resorted to; and after some day's consideration, sir Walter Ogilvie, well aware of the difficulties of the case, and of its great uncertainty, yet justly weary of the long life of torment which he had led, was resolved to have the operation performed, for the chance which it afforded of bringing a relief, only otherwise to be attained by death. It was accordingly performed by Mr. Cline, on the 11th of August.

He was placed in the usual position, and the ordinary ligatures were applied; but the paralysis of the lower limbs rendered it unnecessary to confine them, and they were left at liberty. It was found impossible to pass the staff, or any kind of forceps, into the bladder; but on pressing hard with the finger, part of the stone felt soft, and gave way. This making room for the forceps and scoop, some parts of the concretion were brought away; and, in this manner, about a large teacup-full was extracted. The great mass, however,

continued hard and immovable: it could neither be broken nor shaken from its position; and after trying every instrument, of all powers, the operator was compelled to desist, leaving it (with the exception of the soft matter brought out) in its former state. The patient bore this long process with great fortitude: happily, the paralysis of his lower regions may be supposed to have somewhat diminished the sensibility of the parts. He was, however, completely exhausted, from weakness and fatigue.

Although no hemorrhage, nor any fatal symptom, appeared to result from the operation, it had afforded no sort of relief; and the spasms continued as before. They soon increased in frequency and effect, as he became exhausted, and unable to stand them. He seems to have had a gleam of hope on the morrow of the operation; for he frequently inquired when he might be well enough to have the remainder extracted. But he gradually sunk under the repeated and violent irritations of the spasms. On the eighth day, his pulse became smaller and quicker, and he could scarcely be prevailed on to take any sustenance: on the ninth, he was more impatient, feverish and restless; and on the tenth day, "he desired not to be teased to take any thing more; when covering himself completely with the bed-clothes, he quickly resigned a most singularly miserable existence."

On dissection, the bladder was found to be *quite filled* with a huge calculus, composed of the triple phosphate, which we shall afterwards have occasion to speak of; and of such an unusual proportion of animal matter, that it speedily showed a strong tendency to putridity, and even engendered maggots. It weighed forty-four ounces, or three pounds four ounces, apothecary's weight. Its form was that of an irregular ellipsoid; the larger circumference being sixteen inches, and the smaller fourteen. It had filled the bladder, and taken its shape, but apparently without distending it. The ureters had become enlarged, so as to contain the urine, and act as small bladders, while the large bladder only permitted that fluid to

pass off by means of small furrows which it made in trickling along the surface of the stone, between the stone and the bladder. There appears no reason to doubt, that, in a short time, even this painful and insufficient operation would have become impracticable, by the further enlargement of the stone stopping up the mouths of the ureters; in which event, death must have ensued from suppression.

Such, in its very worst state no doubt, is the dreadful malady which forms the subject of the chymical papers now before us. To these we must now direct our attention; and we can promise the reader sufficient reward for his trouble, if he shall be pleased to follow us through the analysis of their contents.

Mr. Brande's valuable paper, which stands first in the prefixed list, is founded on an examination of the collection of calculi contained in the Hunterian museum. Besides the known richness of that collection, most of the specimens have histories of the case annexed to them, which are obviously of the greatest use in the investigation of the subject. Mr. Brande arranges calculi into four classes: first, those which are formed in the kidneys, and voided without undergoing any change in the passage; second, those which are retained in the kidneys; third, those which are found in the bladder: and, fourth, those which are found in the urethra. On each of these species, our author has made a number of experiments; and the results exhibit a sufficiently distinct account of their chymical analysis. The first class, or calculi of the kidneys, consits almost always of uric acid (the Lithic acid of Scheele) and animal matter; the animal matter is found in various proportions, from one-eighteenth to one-third of the whole compound. Sometimes the acid and the matter are not formed into a solid calculus, but pass off mixed with phosphates. The author, however, is disposed to think that the phosphates are not elaborated in the kidney, but mingle with the calculous matter in its passage. In some few instances, the calculus contains oxalate of lime; but this is very rare. The size of these calculi varies, and one has been des-

cribed by Dr. Heberden, so large as to weigh twenty-eight grains; but it may have been detained on its passage, and so augmented in bulk. The largest which Mr. Brande examined only weighed seventeen grains. All the calculi of this class are certainly soluble in pure potash; but it is needless to add, that the caustic qualities of this substance render its application in the living subject impossible.

The second class is formed by the detention of a calculus in the kidney, while either the secretion of uric acid enlarges its bulk, sometimes so as to form a complete cast of the pelvis of the kidney; or the stream of urine to which it is exposed deposits the phosphates upon the original nucleus of uric acid and animal matter. In the one case, we have an enlarged calculus of the first class, in the other a calculus of the third class, only detained in the kidney.

When the uric calculus passes into the bladder, and is there detained; or when a calculus of oxalate of lime comes down from the kidney; or when portions of sand or animal mucus are deposited in the bladder; or when any extraneous substance has found its way thither and is detained—in all these cases, that is, on all these substances as nuclei, there are formed calculi (commonly termed stones in the bladder) with greater or less rapidity, according to the tendencies of the constitution. They may be ranged into three subdivisions: those composed chiefly of the uric acid; those chiefly composed of phosphorates (the ammoniaco-magnesian phosphate, or phosphate of lime, or a mixture of the two;) and those containing oxalate of lime, which are commonly termed mulberry calculi. Messieurs Fourcroy and Vauquelin, and Dr. Pearson, having found the greater number of calculi to be composed of uric acid (that is to say, belonging to the first of these subdivisions,) our author was surprised to find that, in the Hunterian collection, this proportion did not hold; for, of a hundred and fifty which he examined, only 16 were composed of uric acid wholly (beside the animal matter:) 45 of that acid with a small portion of phosphates: and 5 of the acid with phosphates and nuclei of oxalate of lime; while 66

contained phosphates with a small admixture of uric acid; 12 the phosphates entirely; and 6 chiefly oxalate of lime.—Observing that a greater loss was sustained in obtaining uric acid from calculi in the bladder, than from calculi in the kidney, Mr. Brande was led to conclude that the former contained urea. With a view to ascertain this point, he made some experiments, and found that an uric calculus, weighing 60 grains, contained 5.2 of urea and muriate of ammonia, 6 of ammoniaco-magnesian phosphate, and 48.8 of uric acid: but no urate of ammonia could be detected: from whence, and from other experiments, our author infers, that this substance, when supposed to exist in calculi, owes its origin to the development of ammonia, and its recomposition with uric acid in the course of the processes of decomposition. The mulberry calculi (which are rare) contain, in general, a considerable admixture of phosphates and uric acid with the oxalate of lime. The very large calculi often contain an agglomeration of different species of calculi. Mr. Brande examined one, of twenty-three ounces and twenty-six grains, which had a nucleus of uric acid, surrounded with a considerable quantity of very pure ammoniaco-magnesian phosphate: the mass of the stone consisted of mulberry calculus. Another, weighing 15 1-2 ounces, had a nucleus of uric calculus, round which was a mass of ammoniaco-magnesian phosphate, interesected with laminæ of uric acid. It is rare to find calculi composed of four distinct substances. Our author saw one which had four separate layers, or strata, of uric acid, oxalate of lime, ammoniaco-magnesian phosphate, or phosphate of lime. He examined four calculi, the nuclei of which were extraneous substances, viz. a garden pea, a needle, a hazel nut, and part of a bougie. Besides the animal matter, the first calculi contained phosphates only; the second, phosphates and oxalate; and the other two were also destitute of uric acid.

The fourth class of calculi, those found in the urethra, offered no peculiarities to the observer. Indeed, the classification of Mr. Brande does not refer so much to any distinct

properties of the substances arranged, as to their position and accidents; their *habitat*, as the botanists express it; for their properties seem to depend merely upon their exposure to the urine in the different stages of its elaboration; nor is there any manifest variety of circumstances affecting their formation, except in so far as they are found in streams of that fluid in its nascent or in its perfect state. The calculi in the urethra accordingly, are only small calculi which, passing from the bladder, have been detained and lodged in the membranous part of that canal, and, there, have been exposed to the same process of slow accretion, from the passage of urine, which they would otherwise have undergone, from lying in the bladder exposed to the action of the same fluid. One of the calculi of this description, which Mr. Brande examined, was of a singular appearance. It was a perfect sphere, of about half an inch in diameter, coated with small regular crystals of the ammoniaco-magnesian phosphate in its purest state.

Our author closed his experiments with examining the calculi of different animals. In the horse, sheep, rhinoceros and rabbit, the calculi consisted of the phosphates in part, with animal matter and carbonate of lime. In the dog, no carbonate of lime was found; and, in the hog and ox, no phosphates; the calculus being wholly composed of carbonate of lime and animal matter. No uric acid nor oxalate seems to have been detected in any of the specimens. Some of them were of a large size: a calculus from an old dog weighed sixteen ounces.

From this indication, it is very evident that one conclusion may safely be drawn: that, in most instances, the stone is connected with the formation of uric acid. Sometimes that acid forms calculi in the kidneys, which after creating sufficient torment in that region, pass into the bladder, and become the nuclei of still larger, more painful, and more fatal accretions of other substances; while, in many instances, the same acid continues to augment the mass, either alone or combined with other substances. To prevent its formation, then, becomes

a most important object; and both Mr. Brande and Mr. Home propose, for this purpose, the exhibition of some alkaline earth, which may neutralize and carry it off. But, before adverting to this preventive remedy, we may notice the additional proofs adduced, in the papers now under review, to evince the hopelessness of any attempts to solve the stones already formed, at least in the present state of chymical science:

In arguing this point, Mr. Brande (or, as Mr. Home always terms him, Mr. W. Brande) proceeds more by principle; his patron Mr. Home comes forward to his protection with cases. Mr. Brande clearly shows, that an alkali administered to a calculous patient stands no chance of reaching the uric concretion in a caustic state, for the urine contains both phosphoric and carbonic acid uncombined. But experiment clearly shows, that neither carbonates nor subcarbonates exert any sensible action on uric acid: in other words (as we apprehend) the affinity of the uric acid for alkalies is weaker than the affinity of crabonic acid for the same bodies: therefore, alkaline liquors cannot act as solvents of the uric calculi. On the other hand, if acids be administered with a view of attacking the phosphates, the formation of the uric acid is greatly favoured. This, Mr. Brande's experiments have also taught him. And as for the injection of solvents through the urethra into the bladder, he justly observes, that although we were not ignorant (as we are) of the kind of calculus in each case, the frequent introduction of instruments into the bladder, and the long continuance of the operation, would form insuperable obstacles to this mode of treatment, which, though recommended by great names, has always been speedily relinquished when tried.

Now come Mr. Home's cases: which plainly demonstrate, that where the use of alkaline medicines was supposed to have relieved the patient by dissolving the calculus, on examination no such effect was found to be produced. In two cases, where the violence of the complaint appeared to have

subsided, the dissolution was taken for granted, and ascribed to alkaline medicines; but, on dissection, the calculi were found in great size, only imbedded in cysts, from the enlargement of the prostrate gland, which often takes place late in life. In some instances, again, the increase of calculous concretions was found to proceed rapidly, while the patients were going on with courses of alkaline medicines. One person took these remedies for four or five years, and at his death the bladder was found nearly filled with light, spongy calculi of different sizes, no less than three hundred and fifty in number. Another, who had taken soda, both mild and caustic, for some months, and then submitted to the operation on the symptoms increasing, was found to have a calculus, which was surrounded with a coat of triple phosphate, one tenth of an inch thick, the rest being a mixture of uric acid and phosphates; from whence it is fair to infer, that the alkali increased the formation of triple phosphate, though it checked the production of uric acid. In a third case, of a very virulent nature, the exhibition of alkalies did not even prevent the formation of uric concretions. Mr. Brande's experiments however, in 1808, having rendered it highly probable that, in the great majority of cases, alkaline medicines would operate powerfully in checking the growth of uric calculi, by neutralizing the uric acid before it could form a concretion, this inquiry has lately been resumed by that industrious and skilful observer, with the aid and advice of Mr. Home; and the result of their joint labours is given in the fourth of the papers examined at the head of this article.

Upon consultation with Mr. Hatchett, they were led to fix upon magnesia, in preference to the other alkaline substances; and the event appears to have justified this very natural and judicious choice. Some preliminary trials showed, that the mild, safe, and easily prepared earth in question, diminished the quantity of uric acid in urine, which contained a larger proportion of it than any of the alkalies, however copiously administered. But this point is of such primary importance,

that we must notice the four cases in which a further examination of the matter was prosecuted.

The first was that of a gentleman sixty years old, who, apparently from the habit of drinking acid liquor, had acquired an uncommon tendency to secrete uric acid, and had his urine constantly mixed with that substance, in the form of red sand or crystals. He took, first, the subcarbonate of soda, and then of potash, without any amendment from the former, and with but a slight relief from the latter. His urine being previously examined, he was ordered to take fifteen grains of magnesia three times a day, and in a week the uric concretions diminished sensibly: in three weeks, they were only observed occasionally. The same medicine has been continued for eight months, and no calculi have been voided; nor has there been any material deposit in the urine. He has also been cured of heartburn, and other stomach complaints. Another gentleman, of about forty years of age, who suffered greatly from a similar complaint, and tried the alkalies in vain, has been wholly cured by a six weeks' course of magnesia, in the quantity 'of twenty grains every night and morning,' (we wish Mr. Brande would say more distinctly whether he means *twenty* grains a day, or *forty*,) without any change of regimen whatever. The third case, is that of a gentleman forty-three years old, who has, for about a year, succeeded in driving away repeated attacks of the uric acid, by taking magnesia for a few weeks at a time, when the fit comes on. The fourth case is that of a more confirmed calculous tendency, which has nevertheless yielded, in a great degree, to the free use of magnesia; with this addition, that since the patient began it, he has been free from gout, contrary to his former habits.

The different qualities of magnesia and the alkalies thus observed, led our author to make some experiments on their respective effects upon *healthy* urine. Subcarbonate of potash and soda occasioned a copious precipitation of the phosphates in the urine, during the first two hours after these salts were

taken into the stomach; and, after that, no further effect was produced. The same alkalies, saturated with carbonic acid, threw down the phosphates less copiously and rapidly. Magnesia, administered in the same circumstances, produced a much smaller and slower precipitation. Instead of this effect reaching its maximum in a quarter of an hour, as it did in the case of the alkalies, it did not reach that point in less than six hours; and on this circumstance our author concludes, that its value in calculous disorders chiefly depends. Lime, either in the caustic or mild form, produces a very slight effect; and its nauseous taste, as well as difficulty of administering it in sufficient quantities, manifestly preclude its use in this disease. In one case, carbonic acid, exhibited in aerated water, was found to keep the phosphates (which the urine was greatly charged with) in a state of solution; and, when left off, those salts were again voided in their solid shape.

Thus far, then, we think, it may safely be hoped, at least with as little risk of disappointment as usually attends our speculations in medical science, that some light has been thrown on the method of treating a disease, of all others which prey upon the body, the most inimical to the happiness and comfort of man. Where the object is so valuable, it is natural for us to feel uncommonly anxious and distrustful, as well as more than usually eager in our wishes for its accomplishment. Instead, therefore, of being satisfied with what Mr. Brande and Mr. Home have already effected, we would expect them to persevere in their experimental inquiries: multiplying the number of their observations on real cases: and, as it were, leaving no stone unturned to attain the complete mastery of this great problem. Much will be done for medicine, if they should go no further than perfecting the preventive application of magnesia to the uric calculus. A great number of calculous cases, of the worst description, will be prevented: those, to wit, in which the stone in the bladder is formed on a uric nucleus. Many others of the same class will be greatly relieved. Those, in which composite calculi

consist partially of uric acid; and all that class of complaints with which the region of the kidneys is afflicted, by the uric concretions formed there, will be entirely removed. Should such a step in the healing art be made, we need scarcely despair of living to see the phosphates themselves attacked by it, and yielding to some equally simple and safe remedy.

While we are, however, indulging in these visions, and anticipating the final eradication of all calculous disorders, we unluckily cast our eye on the last of the papers which are now before us, and find, that Dr. Woollaston has been discovering a new species of urinary calculus, at the very moment that his learned brethren were occupied in extirpating the already sufficiently numerous specimens of the old catalogue. This new species is extremely rare, our author having only met with it twice. It resembles the triple phosphate more than any other kind; but is more compact, and consists, not of laminæ, but of a confused mass of chrystals, having a yellowish lustre and semitransparency; but it is formed of a peculiar substance. Dr. Woolaston made as many experiments on this substance, as the limited quantity of it in his possession would allow. When burnt, it gives a smell quite peculiar to itself. When distilled, it gives a fetid carbonate of ammonia, and an animal oil, leaving a spongy coal. It is readily dissolved by all the pure alkalies and by lime water: it is even solved by the carbonates of potash and soda. The acids dissolve it also, except the citric, tartaric, and acetic. Neither water, alcohol, nor saturated carbonate of ammonia, dissolve this singular substance. From its disposition to unite with both acids and alkalies, Dr. Woollaston suspects it to be an oxide; which is confirmed by its forming carbonic acid in distillation; that is to say, if we take it for granted, as he seems to do, that this acid does not exist ready formed in the mass. If it does contain oxygen, our author admits, it must be in a quantity insufficient to give it acid properties; for it produces no effect on the color of litmus paper. On the supposition of its being an oxide, and to distinguish it from other bodies of that class,

Dr. Woollaston proposes to name it the *cystic oxide*, on account of its having hitherto been only found in the bladder.

This excellent chymist concludes his paper with some curious observations on the connexion between the production of uric acid in birds, and their food. The white matter contained in their urine, and voided along with their dung, was found by M. Vauquelin to consist principally of that acid; and our author examined with some care in what manner its quantity was affected by the diet of those animals. In the dung of a goose fed on grass, it only formed 1·200 part; in that of a pheasant fed on barley, it amounted to 1·14; in that of a hen which fed on insects as well as vegetable food, in a barn-yard, it was much more abundant, and mixed with lime. The solid part of the dung of a hawk fed wholly on flesh, was chiefly uric acid; and the evacuation of a gannet, feeding solely on fish, consisted altogether of urine; for the only solid parts were uric acid. "It seems, (our author concludes) deserving of inquiry, what changes might be produced in the urine of any one animal by such alterations of its diet as its constitution would permit: for, as far as any inference can be drawn from these varieties which naturally occur, it would apear that persons subject to calculi consisting of uric acid, as well as gouty persons in whom there is always a redundance of the same matter, have much reason to prefer vegetable diet; but that the preference usually given to fish above other kinds of animal food, is probably erroneous."

The mention of Dr. Woolaston's paper naturally leads us to reflect on the important services which this truly philosophical inquirer has formerly rendered to the branch of science now under consideration. It was his important discovery of the nature of gouty concretions, which first ascertained the analogy long suspected between gout and stone; and, besides giving the first analysis of those concretions which had ever been experimentally attempted, he also first (with the able assistance of Mr. Tennant, a name far too well known to need the humble tribute of our admiration) investigated the com-

position of the calculi, consisting of neutral salts. The discovery of Scheele, perhaps his most important discovery, brought to light the nature of uric calculi; and, at the same time, made us acquainted with a new acid. The existence of this acid was suspected in gouty concretions; and it was generally imagined that they consisted of the acid alone, or combined with animal matter only. Dr. Woollaston showed by experiment that they are composed of uric acid and soda. Until he carried his inquiries into the other classes of calculi, it was only by conjecture or vague analogies that their constitution was known; but to him we owe as perfect an acquaintance with their component parts, as Scheele had left us of the uric calculus. Having thus completed our analysis of the calculi formerly known, he has, in his present communication, brought to light both a new calculus, and a body. His former paper was published in the Philosophical Transactions for 1797; and the one now before us forms an important appendix to it. It is not within our province to take any further notice of the first tract; but we could not conclude this article without recalling it to the recollection of the scientific reader, and reminding him of his great obligations to Dr. Woollaston.*

* See also a valuable collection of historical notices, as well as analytical researches, on urinary concretions, in Dr. George Pearson's paper, Phil. Trans. 1798, Part I.

A review of Dr. Mease's Case of Varicose Aneurism, by John B. Davidge, M. D. Professor of the Institutes or Principles of Physick in the Medical College of Maryland.

MEDICAL cases, collected with assiduity, examined with intelligence, and promulgated with ingenuousness, are ample lessons of instruction, but when they are permitted to be vehicles of error, or channels affording facilities to undijested, hasty speculations, they call irresistibly on our attention, and rouse our vigilance ; they should be scrutinized with rigor, yet with justice, and admitted with the extremest caution ; it becomes a duty to control their influence, and repress their extravagance. Were it so that they would fall into the hands of men of science and observation only, we should give ourselves but little concern about them, but as they are intended to convey, and do convey, doctrines and principles, to minds incompetent to determine on their nature, it would amount to culpable dereliction of the duty we owe both the public and our profession to let them pass unnoticed. In an especial manner are we bound to subject such cases, as are here alluded to, to strict and critical analysis, where they come forth clothed with the authority of character, and enforced by the recommendation of that celebrity which already and deservedly possesses the public confidence.

Had the above case, to be found in the sixth number of the Philosophical Journal and Review of New-York, from the pen of Dr. Mease, been given to the world unaccompanied by the *preface* and *conclusion* by Dr. Dorsey, it would have excited little interest and awakened less curiosity. There is so much of common character about it; so much of what every professional man, at all in the habit of using the lancet, must frequently meet with, that it would have called forth few other sentiments than those of gratitude for the benevolent intentions of the respectable author, and, perhaps, surprise at the minuteness of a detail without the force of conviction or ornament of variety.

Upon reading the prelude, on which the editor of the Journal in a foot-note, makes a judicious remark, we acknowledge we anticipated something curious and valuable; such as, if it did not illustrate what the writer wished to establish, would at least have afforded entertainment to the practitioner and information to the pupil; but, in this our expectation, we were disappointed. The first point, in the detail of the case, deserving of remark, is "The blood at first appeared *very dark*, for I had been in high health."

That the blood when it first flows from a vein, recently opened, should be *very dark*, may be attributed to almost any circumstance rather than to *high health*. We presume the author means by very dark, *unusually* dark.

The blood may be very (unusually) dark from any disease which may so affect the lungs in their function as to prevent the blood from being properly oxigenated; or from the air being cut off from the lungs by submersion, or a cord round the neck. Contrary to what is the import and meaning of the writer's language, the blood is never very, or in any eminent degree, dark, exclusively from high health. It is true it may be very dark when the arm has been bound up any considerable time before the vein be opened, although the persons have been in high health. And we are not a little surprised that a gentleman of the professional respectability of Dr. Mease, should have permitted an idea so unphilosophic to have escaped from his pen.

"In about ten or twelve seconds it (the blood) changed to a florid complexion, and was discharged in jets."

From this the writer wishes it to be inferred that the former quantity, the very dark, was from the vein, and the latter from the artery. And to fortify this suggestion still more, he is careful to mention that it was discharged in jets.

If a ligature be passed round the arm for a short time and then the vein be opened, the blood first discharged will be dark in proportion to the time the ligature was on previous to the opening of the vein; if it be permitted to flow for a

minute or two it becomes of a colour more or less florid; and if the vein opened be the basilick, running immediately over the artery, the blood will be discharged in jets. Nor will those circumstances occur casually; they are almost always uniform.

The blood is dark because it is detained in the veins of the arm, as it were out of the general circulation; it becomes of a florid complexion because there are fresh supplies round from the arteries; and it is discharged in jets because the great artery in the fold of the arm is strongly beating under the lower wall of the basilick vein. There is nothing in all this which can *decisively* point out the artery to be wounded, or should have led a practical man to have suspected such an accident.

"I felt the pulsation of the blood, while flowing from the artery into the vein."

Of this, no doubt, the Doctor felt satisfied, but he will excuse us, we hope, when we acknowledge that we can have no satisfactory conception how, or in what way, he could determine whether the pulsation he felt were of the blood flowing from the artery into the vein, or from the blood flowing along the channel of the artery, beating against the blood effused from the vein. We may repeat, that to determine the pulsation felt to have been from the blood flowing from the artery into the vein, required more nicety and accuracy of touch, than in our estimation could have been boasted by any man, how well soever trained his sensation might have been; and we are still disposed to propound the question: Is the pulsation of the blood while flowing from the artery into the vein obviously different from the pulsation of the blood while flowing in the tube of the artery? and, If it be not indicated by some marked variety, might not the Doctor have been imposed on?

"The *pulsation* of the *blood*, is an unusual expression. We understand the *pulsation* of an *artery*.

"And perceived the thrilling noise from the same cause whenever I put my head under the bed-clothes."

How strongly soever the Doctor was persuaded that he per-

ceived a thrilling noise whenever he put his head under the bed-clothes, we must be indulged in the opinion that there was more of *imagination* than *reality* in the perception. Fancy is sometimes extremely unruly, and at no time more so, than when a man had become persuaded that the main artery of the arm was wounded. Particularly when the subject of this supposed injury is a physician, well acquainted with the consequences of such a wound.

Why could not the thrilling noise be heard except when the Doctor was in bed? Is *darkness* favorable to sound? It is to the exercise of the imagination. Or are vibrations propagated better under the bed-clothes than in the open air of the room? It does not appear that this thrilling noise was heard by either Dr. Physic or Dr. Dorsey.

"I was careful to use my arm as little as possible, for besides the continuance there was a small swelling about the size of a small filbert in the injured part; but about five or six weeks after the accident I imprudently exercised it for half an hour, when the part swelled to the size of a common filbert, became painful and looked red. The pain and redness however soon subsided, but the swelling did not diminish for upwards of two months."

Now we have before us the plain and unadorned fact; the simple description of an ecclymosis compounded with an inflamed and thickened tendon; such as we have all met with so often that we have almost ceased to notice them.

In a genuine varicose aneurism, the blood passes freely and uninterruptedly from the artery into the vein, especially so when a thrilling noise can be heard. If then there be a liberal and ready passage of the blood from the artery into the vein, and afterwards an easy progress on through the vein to the heart, how could an increase of swelling, a redness and pain occur? This pain, this redness, this increase of swelling, argues very strongly, to our understanding, that the case was an ecchymosis combined with inflammation and thickening of the circumjacent substance. Indeed except there be a prompt and ready passage of the blood from the artery into

the vein, the vein can never become varicose; and if there be this ready passage of the blood we cannot understand how, at the distance of six weeks after the injury occurred, inflammation could take place; but that it did take place is obvious from the phenomena of redness, pain, and swelling.

"But, says the Doctor, the swelling did not diminish for upwards of two months."

If the case were a genuine case of varicose aneurism, what could cause the *swelling* to diminish at all, until the aperture in the artery were closed? The vein being once varicose, would continue to be so, and, indeed, increase in volume, so long as the blood should have free access to it from the artery.

"Doctor Physick having carefully examined the arm, was perfectly convinced of the existence of varicose aneurism and did not entertain the faintest hope of a spontaneous cure."

The opinions of Dr. Physick are respectable, and we are at all times gratified in paying regard to them; but we are desirous it should be understood that the opinions of that gentleman, like those of every other gentleman of science and observation, must be considered as opinions, *mere opinions*, and can by no means be substituted for facts. And we are the more unwilling, in the present instance, to admit the substitution of *opinions* for *facts*, as we are informed that the able Physician referred to had not the faintest hope of a spontaneous cure. That is, neither his reading or experience had ever furnished him with a spontaneous cure of a varicose aneurism, and conceiving this to be one, he had no ground on which he could bottom his hope.

If neither the fair pages of surgical history, nor the honest experience of any judicious Surgeon, can furnish a solitary case of a spontaneous cure of a varicose aneurism, or indeed any other, without the obliteration of the diameter of the artery wounded, ought there not to be some little hesitation in making up a decisive opinion in the present case? But even granting that the artery was wounded, that it was an une-

quivocal aneurism, what evidence has Dr. Dorsey, from which he can reasonably or justly conclude that "the artery ever since has been pervious."

"At this time and ever since the cure, the artery has been pervious, and pulsates like that in the other arm at the elbow and wrist."

In this concluding sentence Dr. Dorsey indulges himself in an unreserved expression of a positive opinion on a subject, respecting which he could have had no decisive or positive knowledge. We will grant however to show more plainly the inconsiderateness of the Doctor's conclusion, that the artery was wounded, and that there has been a spontaneous cure. But does it follow as a necessary consequence from a spontaneous cure that the artery must be pervious? There are numerous instances of spontaneous cures of aneurism on record, but in all there was an obliteration of the cavity of the artery. On the contrary there is no case where the wound in the wall of the artery has been closed by accident or by nature, leaving the artery *pervious*.

The lumen of an artery may be obliterated to a certain extent and yet, in consequence of the touch by the finger, being confused by the pulsation of the artery above, and below the point of union or consolidation of the sides of the artery, no satisfactory or unexceptionable deduction can be established.

With regard to the pulsation at the wrist, on which some stress is laid, we hope it is unnecessary for us to suggest, to the learned gentleman, that the perviousness of the main artery at the fold of the arm is by no means necessary. He well knows, as every surgeon must know, that an artery pulsates, when supplied by anastamosing branches, its trunk being tied or cut asunder, as regularly and nearly or quite as forcibly, as when its diameter is free and open throughout.

It appears to be forgotten that there was, and that too for a considerable time a compress on the tumour. What prevented the sides of the artery from being forced into contact by a compress continued for a series of days? And if forced into contact, what prevented a union?

But Dr. Dorsey decisively and unhesitatingly avers that the artery is pervious. And by what sensible means has he ascertained this? we know of but one mean, unequivocal and satisfactory, by which this point can be settled, that is *injection.*

Surely gentlemen must be very solicitous to sustain the notion that wounds in arteries can be cured spontaneously without a closing of their cavites, and that the surgeon need not trouble himself about the ligature or compress which we declare to be *necessary* in all wounded arteries.

We will conclude the present remarks, with hinting to these gentlemen, and those who may fall in with their cases, that, if they do not obliterate the diameters of wounded arteries by compress or ligature, the fate of their patients will remain as sad memorials of the imperfect condition of the surgical art; and we will subjoin that we exceedingly regret, because we highly respect Dr. Dorsey, that, without *positive* proof of any kind, or *uneqvivocal* evidence of any sort, or even *probability*, he has permitted himself to publish the following paragraph: he should have recollected that a sufficient number of lives have already been lost from wounded arteries left to "heal without an obliteration of their cavity."

"In compliance with my promise, I have the pleasure to subjoin the case of anuerism which I consider a *decisive* proof, that *wounded arteries may heal without an obliteration of their cavity.* I fully concur with you in the opinion that such a termination is extremely rare; but those writers who pronounce the thing impossible, are surely mistaken"!!

Medical and Philosophical Intelligence.

Prize Questions for 1812.

THE committee appointed by the corporation of Harvard University, to carry into effect the laudible views of Ward Nicholas Boylston, esq. do hereby engage to deliver to the writers of the best dissertations on each of the following subjects, respectively, a Gold Medal, of the value of thirty-three dollars, or that sum in money, at the option of the successful candidate in each case, viz.

1. On Epilepsy—its varieties, and the mode of treament of each.

2. What substitute can be had for leeches, in order to draw blood gradually, with little or no pain from small surfaces; and which from cheapness and facility of application, in the hand either of the patient or of the medical practitioner, shall be adapted to general use.

3. On the medical uses of carbonic acid gas.

The dissertations must be transmitted (post paid) to Lemuel Hayward, esq. M. D. Boston, on or before the 31st day of December, 1812. Each dissertation must be accompanied with a sealed packet, on the outside of which shall be written some device or sentence, and on the inside, the author's name and place of residence. The same device or sentence must be written on each dissertation as on the packet which is attached to it. Dissertations with the names of the authors affixed to them, will not receive any attention from the committee.

Unsuccessful dissertations shall be returned, if desired, with the sealed packets unopened; or if this be not desired,

the sealed packet shall be destroyed by the committee, without breaking the seal.

LEMUEL HAYWARD,
JOHN WARREN,
JOHN BROOKS,
THOMAS WELSH,
AARON DEXTER,
JOSIAH BARTLETT,
WILLIAM SPOONER,
JOHN FLEET,
JAMES JACKSON,

Boston, May 6. 1812.

Mr. George Myers, of this city, has discovered, in the neighbourhood of Baltimore, a perfect specimen of the *Sulphuret of Molybdæna*, the first we believe that has been found native in the United States.

College of Medicine of Maryland.

At a stated meeting of the Regents, on the 6th of January, 1812, George Brown, M. D. having signified by letter his resignation of the office of President, Charles Alexander Warfield, M. D. was unanimously elected.

At a stated meeting of the Regents, on the 6th of April, the following adjustment of professorships was adopted, viz. James Cocke, M. D. Professor of Anatomy; John B. Davidge, M. D. Professor of the Institutes of Physic; Nathaniel Potter, M. D. Professor of the Practice of Physic; Elisha De Butts, M D. Professor of Chymistry; Samuel Baker, M. D. Professor of Materia Medica; William Gibson, M. D. Professor of Surgery; John B. Davidge, M. D. Professor of Obstetrics, Adjunct Richard W. Hall, M. D.

At a commencement held on the first Monday in May, the degree of Doctor of Physic was conferred on the following

gentlemen, who published and defended their respective Dissertations, viz:

John O'Connor, of Baltimore, on Carcinoma or Cancer.

Charles L. Snyder, of Hampshire county (Virginia,) on Catamenia.

Henry Curtis, of Richmond (Virginia,) on the seat of Animal Life.

Corbin Amos, of Harford county (Maryland,) on Dysentery.

Thomas D. Jones, of Somerset county (Maryland) ——.

The courses of Lectures during the next session of the Classes, will be delivered in the new College, in Lombard street, as the progress of the building justifies the regents in the assurance that suitable apartments will be completed previous to the first of November.

MEDICAL OBITUARY.

At New York, Edward Miller, M. D. physician of the port of New York, and one of the editors of the Medical Repository.

In Harford County, Md. Jacob Hall, M. D.

At George Town, Potomack, William Baker, M. D.

In Calvert County, Md. James Gray, M. D. and Edward Gantt, M. D.

The hurry in which the three first numbers of the **Lycæum** *were printed, occasioned a number of errors, which the reader will please correct, as follows:*

In page 8 line 31, for als read sal.—same page, last line, for me read some.—p 10 l 7, for reent read cast:—p 11 l 10, for relapses read relapse:—p 12 l 16, for enforee read inforce:—l 26, for tremors read tumors:—p 24 l 5, from bottom, for gongrene read gangrene:—p 26 l 16, for coloric read caloric:—p 36 l 26, for exacertation read exacerbation.—p 43 l 23, for epilottis read epiglottis:—p 55 l 25, for collosum read callosum:—p 63 l 4, for pedunculi read pedunculi:—p 66 l 5, for frequetly read frequently:—p 67 l 6 for ainmals read animals: p 83 l 10 for abervations read aberations:—p 84 last line for atatck read attack:—p 85 l 7, for voses read vocis:—p 89 l 12, from bottom for aa read as:—p 93 l 4, for color read colour.—p 96 l 14, for premordia read primordia:—p 106 l 16, for asme read same:—p 109 l 33, for ignated read ignited:—p 158 l 32, for then read than:—p 176 l 3, for then read than:—l 9 for caitilaginious read cartilaginous:—p 210 l 1, for docomposition read decomposition:—p 216 l 18, for theoreetial read theoretical:—p 226 l 11, for prportion read proportion:—p 243 l 28, for modeate read moderate:—p 253 l 9, from bottom, for adhersive read adhesive:—p 257 l 2, from bottom, for high read neighboring:—l 17, from top, for unequivical read unequivocal: p 259 l 4, from bottom for appa read appomatox:—and in l 2, from bottom, for Fridericks read Fredericksburgh:—p 260 l 10, for great read greater:—p 261 l 7, for general read personal:—l 21, for fertalige read fertilize:—p 263 l 8, for granits read garnets:—p 264 l 11, for viridescent read iridescent:—p 265 l 4, of the note for by the Mr. read by Mr.—p 267 l 22, for grangue read gangue:—p 269 l 5, from bottom, for pollucid read pellucid:—p 270 l 10 and 14, for emethyst read amethyst.

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